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THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

OLD AND NEW AIMS AND METHODS OF MORPHOLOGY[1]

"ADDRESS your audience about what you yourself happen to be most interested in, speak from the fullness of your heart and make a clean breast of your troubles." That seemed good advice, and I shall endeavor to follow it, taking for my text old and new aims and methods of morphology, with special reference to resemblances in function and structure on the part of organs and their owners in the animal kingdom. First, however, allow me to tell you what has brought me to such a well-worn theme. Amongst the many impressions which it has been my good luck to gather during my travels in that enchanting country Mexico are the two following:

First, the poisonous coral snakes, *Elaps*, in their beautiful black, red and yellow garb; it varies in detail in the various species of *Elaps*, and this garb with most of the variations too, occurs also in an astonishing number of genera and families of semi-poisonous and quite harmless Mexican snakes, some of which inhabit the same districts. A somewhat exhaustive study of these beauties has shown incontestably that these often astoundingly close resemblances are not cases of mimicry, but due to some other cooperations.

Secondly, in the wilds of the state of Michoacan, at two places, about 20 and 70 miles from the Pacific coast, I myself collected specimens of *Typhlops* which Dr.

[1] Address of the president to the Zoological Section of the British Association for the Advancement of Science, Birmingham, 1913.

Boulenger without hesitation has determined as *Typhlops braminus*. Now, whilst this genus of wormlike, blind little snakes has a wide circumtropical distribution, *T. braminus* had hitherto been known only from the islands and countries of the Indian Ocean basin, never from America, nor from any of the Pacific Islands which possess other kinds of *Typhlops*. Accidental introduction is out of the question. Although the genus is, to judge from its characters, an especially old one, we can not possibly assume that the species *braminus*, if the little thing had made its way from Asia to Mexico by a natural mode of spreading, has remained unaltered even to the slightest detail since that geological epoch during which such a journey could have taken place. There remains the assumption that amongst the of course countless generations of *Typhlops* in Mexico some have hit off exactly the same kind of permutation and combination of those characters which we have hitherto considered as specific of *braminus*, just as a pack of cards may in a long series of deals be dealt out more than once in the same sequence.

The two cases are impressive. They reminded me vividly that many examples of very discontinuous distribution—which any one who has worked at zoogeography will call to mind—are exhibited by genera, families, and even orders, without our knowing whether the groups in which we class them are natural or artificial. The ultimate appeal lies with anatomy.

Introduced to zoology when Haeckel and Gegenbaur were both at their zenith, I have been long enough a worker and teacher to feel elated by its progress and depressed by its shortcomings and failures. Perhaps we have gone too fast, carried along by methods which have yielded so much and therefore have made us expect too much from them.

Gegenbaur founded the modern comparative anatomy by basing it upon the theory of descent. The leading idea in all his great works is to show that transformation, "continuous adjustment" (Spencer), has taken place; he stated the problem of comparative anatomy as the reduction of the differences in the organization of the various animals to a common condition; and as homologous organs he defined those which are of such a common, single origin. His first work in this new line is his classical treatise on the carpus and tarsus (1864).

It followed from this point of view that the degree of resemblance in structure between homologous organs and the number of such kindred organs present is a measure for the affinity of their owners. So was ushered in the era of pedigrees of organs, of functions, of the animals themselves. The tracing of the divergence of homogenous parts became all-important, whilst those organs or features which revealed themselves as of different origin, and therefore as analogous only, were discarded as misleading in the all-important search for pedigrees. Functional correspondence was dismissed as "mere analogy," and even the systematist has learned to scorn these so-called physiological or adaptive characters as good enough only for artificial keys. A curious view of things, just as if it was not one and the same process which has produced and abolished both sets of characters, the so-called fundamental or "reliable" as well as the analogous.

As A. Willey has put it happily, there was more rejoicing over the discovery of the homology of some unimportant little organ than over the finding of the most

appalling unrelated resemblance. Morphology had become somewhat intolerant in the application of its canons, especially since it was aided by the phenomenal growth of embryology. You must not compare ectodermal with endodermal products. You must not make a likeness out of another germinal layer or anything that appertains to it, because if you do that would be a horror, a heresy, a homoplasy.

Haeckel went so far as to distinguish between a true homology, or homophyly, which depends upon the same origin, and a false homology, which applies to all those organic resemblances which derive from an equivalent adaptation to similar developmental conditions. And he stated that the whole art of the morphologist consists in the successful distinction between these two categories. If we were able to draw this distinction in every case, possibly some day the grand tree of each great phylum, may be of the whole kingdom, might be reconstructed. That would indeed be a tree of knowledge, and, paradoxically enough, it would be the deathblow to classification, since in this, the one and only true natural system, every degree of consanguinity and relationship throughout all animated nature, past and present, would be accounted for; and to that system no classification would be applicable, since each horizon would require its own grouping. There could be definable neither classes, orders, families nor species, since each of these conceptions would be boundless in an upward or downward direction.

Never mind the ensuing chaos; we should at least have the pedigree of all our fellow creatures, and of ourselves among them. Not absolute proof, but the nearest possible demonstration that transformation has taken place. Empirically we know this already, since, wherever sufficient material has been studied, be it organs, species or larger groups, we find first that these units had ancestors and, secondly, that the ancestors were at least a little different. Evolution is a fact of experience proved by circumstantial evidence. Nevertheless we are not satisfied with the conviction that life is subject to an unceasing change, not even with the knowledge of the particular adjustments. We now want to understand the motive cause. First What, then How and now Why?

It is the active search for an answer to this question (Why?) which is characteristic of our time. More and more the organisms and their organs are considered as living, functional things. The mainspring of our science, perhaps of all science, is not its utility, not the desire to do good, but, as an eminently matter-of-fact man, the father of Frederick the Great, told his Royal Academicians (who, of course, were asking for monetary help) in the following shockingly homely words: "Der Grund ist derer Leute ihre verfluchte Curieusiteit." This blamed curiosity, the beginnings of which can be traced very far back in the lower animals, is most acutely centered in our desire to find out who we are, whence we have come, and whither we shall go. And even if zoology, considering the first and last of these three questions as settled, should some day solve the problem: Whence have we come? there would remain outside zoology the greater Why?

Generalizations, conclusions, can be arrived at only through comparison. Comparison leads no further where the objects are alike. If, for instance, we restrict ourselves to the search for true homologies, dealing with homogenes only, all we find is that once upon a time some organism has produced, invented, a certain arrangement of *Anlage* out of which that organ arose, the various features of which we have compared in the descendants. Result: we

have arrived at an accomplished fact. These things, in spite of all their variety in structure and function, being homogenes, tell us nothing, because according to our mode of procedure we can not compare that monophyletic *Anlage* with anything else, since we have reduced all the homogenous modifications to one. Logically it is true that there can have been only one, but in the living world of nature there are no such ironbound categories and absolute distinctions. For instance, if we compare the organs of one and the same individual, we at once observe repetition, *e. g.*, that of serial homology, which implies many difficulties, with very different interpretations. Even in such an apparently simple case as the relation between shoulder girdle and pelvis we are at a loss, since the decision depends upon our view as to the origin of the paired limbs, whether both are modified visceral arches, and in this case serially repeated homogenes, or whether they are the derivatives from one lateral fin, which is itself a serial compound, from which, however, the proximal elements, the girdles, are supposed to have arisen independently. What is metamerism? Is it the outcome of a process of successive repetitions so that the units are homogenes, or did the division take place at one time all along the line, or is it due to a combination of the two procedures?

The same vagueness finds its parallel when dealing with the corresponding organs of different animals, since these afford the absolute chance that organs of the same structure and function may not be reducible to one germ, but may be shown to have arisen independently in time as well as with reference to the space they occupy in their owners. As heterogenes they can be compared as to their causes. In the study of the evolution of homogenes the problem is to account for their divergencies, whilst the likeness, the agreements, so to speak their greatest common measure, is *eo ipso* taken to be due to inheritance. When, on the contrary, dealing with heterogenes we are attracted by their resemblances, which since they can not be due to inheritance must have a common cause outside themselves. Now, since a leading feature of the evolution of homogenes is divergence, whilst that of heterogenes implies convergence from different starting-points, it follows that the more distant are these respective starting-points (either in time or in the material) the better is our chance of extracting the greatest common measure out of the unknown number of causes which combine in the production of even the apparently simplest organ.

These resemblances are a very promising field and the balance of importance will more and more incline towards the investigation of function, a study which, however, does not mean mere physiology with its present-day aims in the now tacitly accepted sense, but that broad study of life and death which is to yield the answer to the question Why?

Meantime, comparative anatomy will not be shelved; it will always retain the casting-vote as to the degree of affinity among resemblances, but emphatically its whole work is not to be restricted to this occupation. It will increasingly have to reckon with the functions, indeed never without them. The animal refuses to yield its secrets unless it be considered as a living individual. It is true that Gegenbaur himself was most emphatic in asserting that an organ is the result of its function. Often he held up to scorn the embryographer's method of muddling cause and effect, or he mercilessly showed that in the reconstruction of the evolution of an organ certain features can not have been phases unless they imply physiological continuity.

And yet how moderately is function dealt with in his monumental text-book and how little is there in others, even in text-books of zoology:

> Habt alle die Theile in der Hand,
> Fehlt leider nur das geistige Band—Life!

We have become accustomed to the fact that like begets like with small differences, and from the accepted standpoint of evolution versus creation we no longer wonder that descendants slowly change and diverge. But we are rightly impressed when unlike comes to produce like, since this phenomenon seems to indicate a tendency, a set purpose, a *beau idéal,* which line of thought or rather imperfect way of expression leads dangerously near to the crassest teleology.

But, teleology apart, we can postulate a perfect agreement in function and structure between creatures which have no community of descent. The notion that such agreement *must* be due to blood-relationship involved, among other difficulties, the dangerous conclusion that the hypothetical ancestor of a given genuine group possessed in potentiality the *Anlagen* of all the characters exhibited by one or other of the component members of the said group.

The same line of thought explained the majority of human abnormalities as atavistic, a procedure which would turn the revered ancestor of our species into a perfect museum of antiquities, stocked with tools for every possible emergency.

The more elaborate certain resemblances are the more they seem to bear the hallmark of near affinity of their owners. When occurring in far-related groups they are taken at least as indications of the homology of the organs. There is, for instance, a remarkable resemblance between the *bulla* of the whale's ear and that of the *Pythonomorph plioplatycarpus.* If you homologize the mammalian tympanic with the quadrate the resemblance loses much of its perplexity, and certain Chelonians make it easier to understand how the modification may have been brought about. But, although we can arrange the Chelonian, Pythonomorph and Cetacean conditions in a progressive line, this need not represent the pedigree of this *bulla.* Nor is it necessarily referable to the same *Anlage.* Lastly if, as many anatomists believe, the reptilian quadrate appears in the mammals as the *incus,* then all homology and homogeny of these *bullæ* is excluded. In either case we stand before the problem of the formation of a *bulla* as such. The significant point is this, that although we dismiss the *bulla* of whale and reptile as obvious homoplasy, such resemblances, if they occur in two orders of reptiles, we take as indicative of relationship until positive evidence to the contrary is produced. That this is an unsound method is brought home to us by an ever-increasing number of cases which tend to throw suspicion on many of our reconstructions. Not a few zoologists look upon such cases as a nuisance and the underlying principle as a bugbear. So far from that being the case such study promises much beyond the pruning of our standard trees—by relieving them of what reveal themselves as grafts instead of genuine growth—namely, the revelation of one or other of the many agencies in their growth and structure.

Since there are all sorts and conditions of resemblances we require technical terms. Of these there is abundance, and it is with reluctance that I propose adding to them. I do so because unfortunately some terms are undefined, perhaps not definable; others have not "caught on," or they suffer from that mischievous law of priority in nomenclature.

The terms concerning morphological

homologies date from Owen; Gegenbaur and Haeckel rearranged them slightly. Lankester, in 1870, introduced the terms homogenous, meaning alike born, and homoplastic or alike molded. Mivart rightly found fault with the detailed definition and the subdivisions of homoplasy, and very logically invented dozens of new terms, few of which, if any, have survived. It is not necessary to survey the ensuing literature. For expressing the same phenomenon we have now the choice between homoplasy, homomorphy, isomorphy, heterophyletic convergence, parallelism, etc. After various papers by Osborn, who has gone very fully into these questions, and Willey's "Parallelism," Abel, in his fascinating "Grundzüge der Palæobiologie," has striven to show by numerous examples that the resemblances or "adaptive formations" are cases of parallelism if they depend upon the same function of homologous organs, and convergences if brought about by the same function of non-homologous organs.

I suggest an elastic terminology for the various resemblances indicative of the degree of homology of the respective organs, the degree of affinity of their owners, and lastly the degree of the structural likeness attained.

Homogeny.—The structural feature is invented once and is transmitted, without a break, to the descendants, in which it remains unaltered, or it changes by mutation or by divergence, neither of which changes can bring the ultimate results nearer to each other. Nor can their owners become more like each other since the respective character made its first appearance either in one individual, or, more probably, in many of one and the same homogeneous community.

Homoplasy.—The feature or character is invented more than once, and independently. This phenomenon excludes absolute identity; it implies some unlikeness due to some difference in the material, and there is further the chance of the two or more inventions, and therefore also of their owners, becoming more like each other than they were before.

CATEGORIES OF HOMOPLASY

Isotely.—If the character, feature or organ has been evolved out of homologous parts or material, as is most likely the case in closely related groups, and if the subsequent modifications proceed by similar stages and means, there is a fair probability or chance of very close resemblance. *Isotely:* the same mark has been hit.

Homœotely.—Although the feature has been evolved from homologous parts or material, the subsequent modifications may proceed by different stages and means, and the ultimate resemblance will be less close, and deficient in detail. Such cases are most likely to happen between groups of less close affinity, whether separated by distance or by time. *Homœo-tely:* the same end has been fairly well attained. The target has been hit, but not the mark.

Parately.—The feature has been evolved from parts and material so different that there is scarcely any or no relationship. The resulting resemblance will at best be more or less superficial; sometimes a sham, although appealing to our fancy. *Parately:* the neighboring target has been hit.

EXAMPLES

Isotely:

Bill of the *Ardeidæ balæniceps* (Africa) and *Cancroma* (tropical America).

Zygodactyle foot of *Cuckoos, Parrots, Woodpeckers* (2.3/1.4).

Patterns and coloration of *Elaps* and other snakes.

Parachute of *Petaurus* (marsupial); *Pteromys* (rodent) and *Galeopithecus.*

Perissodactylism of *Litopterna* and Hippoids.

Bulla auris of *Plioplatecarpus* (*Pythonomorphe*) and certain whales; if tympanic = quadrate.
Grasping instruments or nippers in Arthropods: pedipalps of *Phryne;* chelæ of squill; first pair of mantis's legs.
General appearance of moles and *Notoryctes,* if both considered as mammals; of gulls and petrels, if considered as birds.

Homœotely:

Heterodactyle foot of trogons (3.4/2.1).
Jumping foot of *Macropus, Dipus, Tarsius.*
Intertarsal and cruro-tarsal joint.
Fusion and elongation of the three middle metatarsals of *Dipus* and *Rhea.*
Paddles of ichthyosaurs. Turtles, whales, penguins.
"Wings" of pterosaurs and bats.
Long flexible bill of *Apteryx* and snipes.
Proteroglyph dentition of cobras and solenoglyph dentition of vipers.
Loss of the shell of *Limax* and *Aplysia.*
Complex molar pattern of horse and cow.

Parately:

Bivalve shell of brachiopods and lamellibranchs.
Stretcher-sesamoid bone of pterodactyls (radial carpal); of flying squirrels (on pisiform); of *Anomalurus* (on olecranon).
Bulla auris of pythonomorph (quadrate) and whale (tympanic); is *incus* = quadrate.
"Wings" of pterosaurs, or bats, and birds.

The distinction between these three categories must be vague because that between homology and analogy is also arbitrary, depending upon the standpoint of comparison. As lateral outgrowths of vertebræ all ribs are homogenes, but if there are at least hæmal and pleural ribs then those organs are not homologous even within the class of fishes. If we trace a common origin far enough back we arrive near bedrock with the germinal layers. So there are specific, generic, ordinal, etc., homoplasies. The potentiality of resemblance increases with the kinship of the material.

Bateson, in his study of homœosis, has rightly made the solemn quotation: "There is the flesh of fishes . . . birds . . . beasts, etc." Their flesh will not and can not react in exactly the same way under otherwise precisely the same conditions, since each kind of flesh is already biased, encumbered by inheritances. If a certain resemblance between a reptile and mammal dates from Permian times, it may be homogenous, like the pentadactyle limb which as such has persisted; but if that resemblance has first appeared in the Cretaceous period it is homoplastic, because it was brought about long after the class division. To cases within the same order we give the benefit of the doubt more readily than if the resemblance concerned members of two orders, and between the phyla we rightly seek no connection. However, so strongly is our mode of thinking influenced by the principle of descent that, if the same feature happen to crop up in more than two orders, we are biased against homoplasy.

The readiness with which certain homoplasies appear in related groups seems to be responsible for the confounding of the potentiality of convergent adaptation with a latent disposition, as if such cases of homoplasy were a kind of temporarily deferred repetition, *i. e.,* after all due to inheritance. This view instances certain recurring tooth patterns, which, developing in the embryonic teeth, are said not to be due to active adaptation or acquisition but to selection of accomplished variations, because it is held inconceivable that use, food, etc., should act upon a finished tooth. It is not so very difficult to approach the solution of this apparently contradictory problem. Teeth, like feathers, can be influenced long before they are ready by the life experiences of their predecessors. A very potent factor in the evolution of homoplasies is correlation, which is sympathy, just as inheritance is reminiscence. The introduction of a single new feature may affect the whole organism profoundly, and one serious case of isotely may arouse un-

suspected correlations and thus bring ever so many more homoplasies in its wake.

Function is always present in living matter; it is life. It is function which not only shapes, but creates the organ or suppresses it, being indeed at bottom a kind of reaction upon some stimulus, which stimuli are ultimately all fundamental, elementary forces, therefore few in number. That is a reason why nature seems to have but few resources for meeting given "requirements"—to use an everyday expression, which really puts the cart before the horse. This paucity of resources shows itself in the repetition of the same organs in the most different phyla. The eye has been invented dozens of times. Light, a part of the environment, has been the first stimulus. The principle remains the same in the various eyes; where light found a suitably reacting material a particular evolution was set going, often round about, or topsy-turvy, implying amendments; still, the result was an eye—in advanced cases a scientifically constructed dark chamber with lens, screen, shutters and other adjustments. The detail may be unimportant, since in the various eyes different contrivances are resorted to.

Provided the material is suitable, plastic, amenable to prevailing environmental or constitutional forces, it makes no difference what part of an organism is utilized to supply the requirements of function. You can not make a silk purse out of a sow's ear, but you can make a purse, and that is the important point. The first and most obvious cause is function, which itself may arise as an incidental action due to the nature of the material. The oxidizing of the blood is such a case, and respiratory organs have been made out of whatever parts invite osmotic contact of the blood with air or water. It does not matter whether respiration is carried on by ectoor by endodermal epithelium. Thus are developed internal gills, or lungs, both of which may be considered as referable to pharyngeal pouches; but where the outer skin has become suitably osmotic, as in the naked Amphibia, it may evolve external gills. Nay, the whole surface of the body may become so osmotic that both lungs and gills are suppressed, and the creature breathes in a most pseudo-primitive fashion. This arrangement, more or less advanced, occurs in many Urodeles, both American and European, belonging to several subfamilies, but not in every species of the various genera. It is therefore a case of apparently recent isotely.

There is no prejudice in the making of a new organ except in so far that every organism is conservative, clinging to what it or its ancestors have learned or acquired, which it therefore seeks to recapitulate. Thus in the vertebrata the customary place for respiratory organs is the pharyngeal region. Every organism, of course, has an enormous back history; it may have had to use every part in every conceivable way, and it may thereby have been trained to such an extent as to yield almost at once, like a bridle-wise horse to some new stimulus, and thus initiate an organ straight to the point.

Considering that organs put to the same use are so very often the result of analogous adaptation, homoplasts with or without affinity of descent, are we not justified in accusing morphology of having made rather too much of the organs as units, as if they were concrete instead of inducted abstract notions? An organ which changes its function may become a unit so different as to require a new definition. And two originally different organs may come to resemble each other so much in function and structure that they acquire the same definition as one new unit. To avoid this

dilemma the morphologist has, of course, introduced the differential of descent, whether homologous or analogous, into his diagnoses of organs.

The same principles must apply to the classification of the animals. To group the various representative owners of cases of isotely together under one name, simply because they have lost those characters which distinguished their ancestors, would be subversive of phyletic research. It is of the utmost significance that such "convergences" (rather "mergers," to use an administrative term) do take place, but that is another question. If it could be shown that elephants in a restricted sense have been evolved independently from two stems of family rank, the convergent terminals must not be named *Elephantinæ*, nor can the representatives of successive stages or horizons of a monophyletic family be designated and lumped together as subfamilies. And yet something like this practise has been adopted from Cope by experienced zoologists with a complete disregard of history, which is an inalienable and important element in our science.

This procedure is no sounder than would be the sorting of our Cartwrights, Smiths and Bakers of sorts into as many natural families. It would be subversive of classification, the aim of which is the sorting of a chaos into order. We must not upset the well-defined relative meaning of the classificatory terms which have become well-established conceptions; but what such an assembly as the terminal elephants should be called is a new question, the urgency of which will soon become acute. It applies at least to assemblies of specific, generic and family rank, for each of which grades a new term, implying the principle of convergence, will have to be invented. In some cases geographical terms may be an additional criterion. Such terms will be not only most convenient, but they will at once act as a warning not to use the component species for certain purposes. There is, for instance, the case of *Typhlops braminus*, mentioned at the beginning of this address. Another case is the dog species, called *Canis familiaris*, about which it is now the opinion of the best authorities that the American dogs of sorts are the descendants of the coyote, while some Indian dogs are descendants of a jackal, and others again are traceable to some wolf. The "dog," a definable conception, has been invented many times, and in different countries and out of different material. It is an association of converged hetergeneous units. We have but a smile for those who class whales with fishes, or the blindworm with the snakes; not to confound the amphibian Cœcilians with reptilian *Amphisbænas* requires some training; but what are we to do with creatures who have lost or assimilated all those differential characters which we have got used to rely upon?

In a homogeneous crowd of people we are attracted by their little differences, taking their really important agreements for granted; in a compound crowd we at once sort the people according to their really unimportant resemblances. That is human nature.

The terms "convergence" and "parallelism" are convenient if taken with a generous pinch of salt. Some authors hold that these terms are but imperfect similes, because two originally different organs can never converge into one identical point, still less can their owners whose acquired resemblance depresses the balance of all their other characters. For instance, no lizard can become a snake, in spite of ever so many additional snake-like acquisitions, each of which finds a parallel, an analogy in the snakes. Some zoologists therefore prefer contrasting only parallelism and

divergence. A few examples may illustrate the justification of the three terms. If out of ten very similar black-haired people only two become white by the usual process, whilst the others retain their color, then these two diverge from the rest; but they do not, by the acquisition of the same new feature, become more alike each other than they were before. Only with reference to the rest do they seem to liken as they pass from black through gray to white, our mental process being biased by the more and more emphasized difference from the majority.

10 *Ax Bx Cx D E F*
9
8
7
6
5
4
3
2 *Ax Bx*
1 *A B C D E F*

Supposing *A* and *B* both acquire the character *X* and this continues through the next ten generations, while in the descendants of *C* the same character is invented in the tenth generation, and whilst the descendants of *D, E, F* still remain unaltered. Then we should be strongly inclined, not only to key together $C(x/10)$ with A $(x/10)$ and $B(x/10)$, but take this case for one of convergence, although it is really one of parallelism. If it did not sound so contradictory it might be called parallel divergence. The inventors diverge from the majority in the same direction: Isotely.

Third case: Ten people, contemporaries, are alike but for the black or red hair. Black *A* turns white and Red *E* turns white, not through exactly identical stages, since *E* will pass through a reddish gray tinge. But the result is that *A* and *E* become actually more like each other than they were before. They *converge,* although they have gone in for exactly the same divergence with reference to the majority.

In all three cases the variations begin by divergence from the majority, but we can well imagine that all the members of a homogeneous lot change orthogenetically (this term has been translated into the far less expressive "rectigrade") in one direction, and if there be no lagging behind, they all reach precisely the same end. This would be a case of transmutation (true mutations in Waagen's and Scott's sense), producing new species without thereby increasing their number, whilst divergence always implies, at least potentially, increase of species, genera, families, etc.

If for argument's sake the mutations pass through the colors of the spectrum and if each color be deemed sufficient to designate a species, then, if all the tenth generations have changed from green to yellow and those of the twentieth generation from yellow to red, the final number of species would be the same. And even if some lagged behind, or remained stationary, these epistatic species (Eimer) are produced by a process which is not the same as that of divergence or variation in the usual sense.

The two primary factors of evolution are environment and heredity. Environment is absolutely inseparable from any existing organism, which therefore must react (adaptation) and at least some of these results gain enough momentum to be carried into the next generation (heredity).

The life of an organism, with all its experiments and doings, is its ontogeny, which may therefore be called the subject of evolution, but not a factor. Nor is selection a primary and necessary factor, since, being destructive, it invents nothing. It accounts, for instance, for the composition of the present fauna, but has not made its components. A subtle scholastic insinu-

ation lurks in the plain statement that by ruthless elimination a black flock of pigeons can be produced, even that thereby the individuals have been made black. (But of course the breeder has thereby not invented the black pigment.)

There can be no evolution, progress, without response to stimulus, be this environmental or constitutional, *i. e.*, depending upon the composition and the correlated working of the various parts within the organism. Natural selection has but to favor this plasticity, by cutting out the non-yielding material, and through inheritance the adaptive material will be brought to such a state of plasticity that it is ready to yield to the spur of the moment, and the foundation of the same new organs will thereby be laid, whenever the same necessity calls for them. Here is a dilemma. On the one hand the organism benefits from the ancestral experience, on the other there applies to it de Rosa's law of the reduction of variability, which narrows the chances of change into fewer directions. But in these few the changes will proceed all the quicker and farther. Thus progress is assured, even hypertely, which may be rendered by "overdoing a good thing."

Progress really proceeds by mutations, spoken of before, orthogenesis, and it would take place without selection and without necessarily benefiting the organism. It would be mere presumption that the seven-gilled shark is worse off than its six- or five-gilled relations; or to imagine that the newt with double trunk-veins suffers from this arrangement, which morphologically is undoubtedly inferior to the unpaired, azygous, etc., modifications. The fact that newts exist is proof that they are efficient in their way. Such orthogenetic changes are as predictable in their results as the river which tends to shorten its course to the direct line from its head waters to the sea. That is, the river's entelechy is no more due to purpose or design than is the series of improvements from the many gill-bearing partitions of a shark to the fewer, and more highly finished comb-shaped gills of a Teleostean fish.

The success of adaptation, as measured by the morphological grade of perfection reached by an organ, seems to depend upon the phyletic age of the animal when it was first subjected to these "temptations." The younger the group, the higher is likely to be the perfection of an organic system, organ or detail. This is not a platitude. The perfection attained does not depend merely upon the length of time available for the evolution of an organ. A recent Teleostean has had an infinitely longer time as a fish than a reptile, and this had a longer time than a mammal, and yet the same problem is solved in a neater, we might say in a more scientifically correct way by a mammal than by a reptile, and the reptile in turn shows an advance in every detail in comparison with an amphibian, and so forth.

A few examples will suffice:

The claws of reptiles and those of mammals; there are none in the amphibians, although some seem to want them badly, like the African frog *Gampsosteonyx,* but its cat-like claws, instead of being horny sheaths, are made out of the sharpened phalangeal bones which perforate the skin.

The simple contrivance of the rhinocerotic horn, introduced in Oligocene times, compared with the antlers of Miocene *Cervicornia* and these with the response made by the latest of Ruminants, the hollow-horned antelopes and cattle. The heel-joint; unless still generalized, it tends to become intertarsal (attempted in some lizards, pronounced in some dinosaurs and in the birds) by fusion of the bones of the

tarsus with those above and below, so that the tarsals act like epiphysial pads. Only in mammals epiphyses are universal. Tibia and fibula having their own, the pronounced joint is cruro-tarsal and all the tarsals could be used for a very compact, yet non-rigid arrangement. The advantage of a cap, not merely the introduction of a separate pad, is well recognized in engineering.

Why is it that mammalian material can produce what is denied to the lower classes? In other words, why are there still lower and middle classes? Why have they not all by this time reached the same grade of perfection? Why not indeed, unless because every new group is less hampered by tradition, much of which must be discarded with the new departure; and some of its energy is set free to follow up this new course, straight, with ever-growing results, until in its turn this becomes an old rut out of which a new jolt leads once more into fresh fields.

H. F. Gadow

THE NEW RELATIVITY IN PHYSICS

Ever since Newton's corpuscular theory of light was supplanted, early in the nineteenth century, by the theory that light travels in waves through ether as sound through air, physicists have been endeavoring to obtain direct experimental evidence about this invisible, imponderable ether.

The earth sweeps through space with a velocity of about 2,000 miles a minute; if ether fills all space, it should be possible with the delicate instruments now in our possession to detect an ether drift, an optical effect caused by the motion of the earth through the ether.

Among others, Professors Michelson and Morley[1] tried to detect this ether drift experimentally, but obtained purely negative results. Although they failed to get evidence of an ether, they did obtain new physical facts of an even greater importance, which have caused us to readjust our concepts of space and time.

Let us assume that the sun and earth are at rest in space; it then takes a beam of light about eight minutes to travel through space from the sun to the earth.

If we assume that both sun and earth are in uniform translation through space, that is, that both are in motion along the same straight line, we would expect, since the velocity of light can not be increased or diminished by motion of its source, that a light beam would be longer on its way from sun to earth when it travels in the direction of the motion, and that the light beam would be a shorter time on its way when it travels counter to the motion; in traveling with the motion the light beam would overtake the earth; when the direction of the motion is reversed, earth and light flash would meet.

These deductions, according to the principle of relativity, are not valid, for the facts presented by Michelson's experiments show us that the number of seconds that a light flash is on its way can neither be increased nor diminished when the interstellar space through which the light has to travel is arbitrarily increased or diminished by giving source and observer the same uniform translation.

Newton based his mechanics upon *absolute* space and time,[2] "not that which the vulgar associate with sensible objects." Clerk Maxwell[3] said: "All our knowledge, both of time and place, is essentially relative." Yet he could not free himself from the Newtonian mechanics, and it was not until 1905 that Albert Einstein[4] repudiated the word *absolute*, and out of the "vulgar" ideas of space and time developed the modern theory of relativity. Einstein was then an employee in the patent office at Bern, and it is but fitting that in Switzerland, which has furnished the world with so many timepieces, new thoughts with

[1] *Silliman's Journal*, 34: 337, 1887.

[2] Newton, "Principia," 1: 8, 1822.

[3] Maxwell, "Matter and Motion," p. 30 (Van Nostrand ed., 1892).

[4] *Annalen der Physik*, 17: 905, 1905; *Jahrbuch der Radioaktivitaet und Electronik*, 4: 411, 1907.

respect to the measurement of time should crystallize, and a new time concept be found.

Any regular process of nature may serve as a measure of time; for example, the fall of sand in the hour-glass, the swing of the pendulum, the sun dial, or to be more modern, an ideal watch which is regulated by a perfect spring and balance wheel. Let us imagine we have two perfect watches, one in San Francisco, the other in New York. How can we synchronize or set them so that both will indicate the same instant of time? To synchronize them both at the factory, and send one to New York and the other to San Francisco, will not do, as we shall see later. Since experiment appears to justify the assumption that the velocity of light through interstellar space is always the same, let us use a light flash to synchronize the watch at San Francisco with the one at New York.

At twelve o'clock the observer in New York sends a light flash to San Francisco, where a mirror immediately reflects it back to him; he finds it took thirty thousandths of a second for the light signal to travel to San Francisco and back; he reasons, therefore, that it took fifteen thousandths of a second to travel one way; he then writes the observer in San Francisco to set his watch at twelve o'clock plus fifteen thousandths of a second, as soon as the light flash again sent from New York at twelve o'clock reaches him; the two watches are then in synchronism for observers at these two stations. The simultaneity of an occurrence at New York with one at San Francisco can then be established by the two synchronized watches. The connotation of the word *simultaneity* thus becomes very definite.

In order to bring out nature's facts with regard to time and space, which Einstein has so clearly presented in mathematical form, we have built a model, constructed briefly, as follows: A triple lead-screw, eight feet long, gives motion to the upper or moving system when the crank at the right of the model is in motion. By throwing in the proper gearing at the crank shaft, a second lead-screw supplies motion (toward right or left) to a light particle (L), in the model, a pocket electric lamp resting upon a traveling nut. Two worm wheels meshing with the first lead-screw operate the hands of the lower or stationary clocks, while a pair of spur gear acting as pinions upon a stationary rack move the clock hands of the system in translation when the latter is in motion.

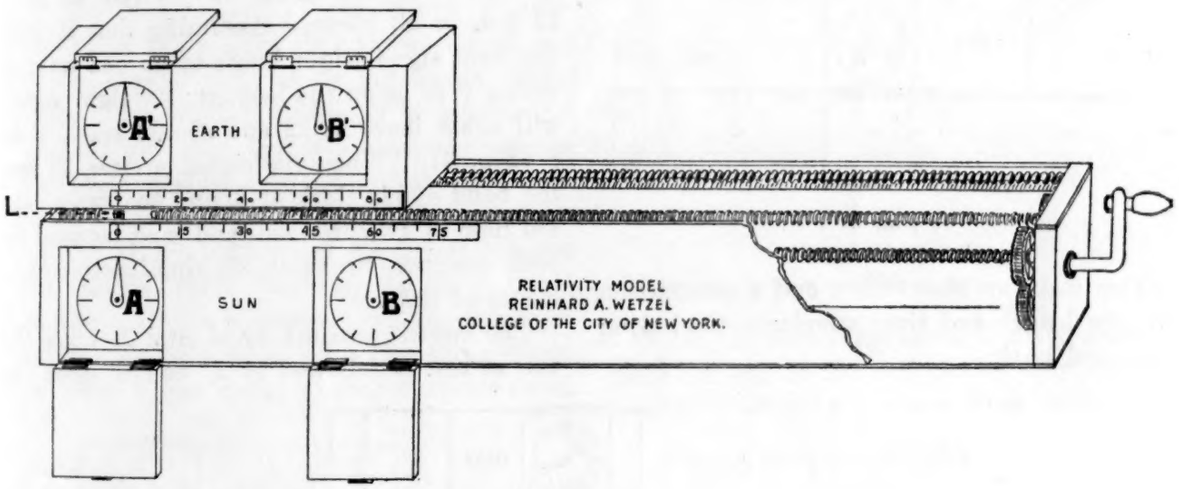

FIG. 1

Following the method of Emil Cohn, of Strassburg,[5] we shall speak of the stationary system as the sun; the two sun clocks are fixed to the sun and are sixty sun miles apart; at each clock station is an observer, sun-man A at the zero station, and sun-man B at the sixty-mile station.

[5] "Himmel und Erde," 23: 117, 1911.

Similarly the moving system may represent an earth always in uniform translation with respect to the sun. At two stations upon the earth sixty earth miles apart are fixed a clock and an observer. A sun-man can see only one earth clock and earth-man at one time,

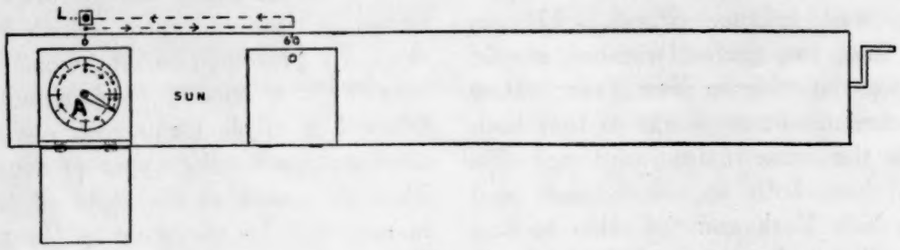

Fig. 2

namely, at the instant earth-man passes sun-man, and *vice versa*. Also, only at the instant an earth-man passes a sun-man can

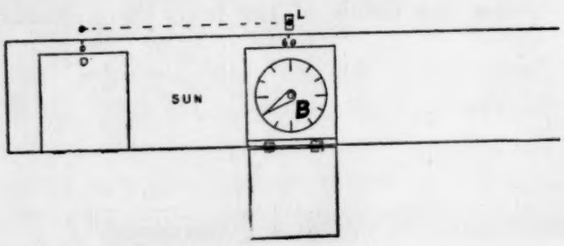

Fig. 3

either make an observation and a comparison of the length and time standards used upon sun and earth.

light particle in the ratio of two to three. We must, therefore, interpret our model as a magnifier of nature's facts with respect to space and time.

Suppose A and B upon the sun wish to synchronize their two clocks which are exactly alike in every respect and perfect mechanisms. When A's clock reads 12 (Fig. 2), he sends out the light signal which, reflected by the mirror at the 60-mile station, returns after the hand of A's clock has moved through $12+4$, or 16 hours. Assuming that it took the light signal eight hours to go one way, A writes B to be on the lookout; the light signal will again leave at 12 and should reach B at 8 (Fig. 3). When the signal arrives, B sets the hand of his clock at 8, and the two clocks are now in synchronism, and may be used by both observers to establish simultaneous moments of time.

The course pursued by A and B upon the sun at rest is followed by A^1 and B^1 upon the

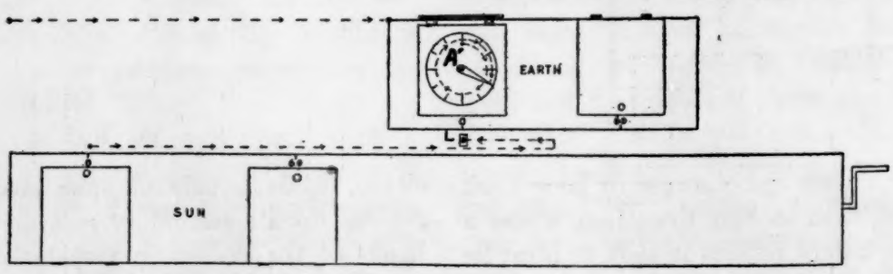

Fig. 4

As the naturalist's picture of a microorganism misrepresents nature as to size, so our model is, in this respect, also a "nature-faker." The orbital velocity of our earth is to the speed of light in interstellar space as one is to ten thousand; but the model arbitrarily represents the earth's velocity to the velocity of the earth in motion. With their own foot rule A^1 and B^1 have placed their two clocks sixty miles apart, and they too (Fig. 4) find that sixteen hours is required for the passage of the light signal from A^1 to B^1 and return, and that B^1 must set the hand of his clock at 8 (Fig. 5) when the light signal sent by A^1 at 12 reaches

him, in order to set their clocks in synchronism.

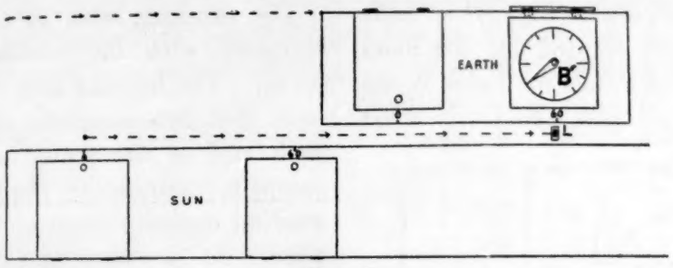

FIG. 5

We are now in a position to measure the velocity of light in both systems, and find that in each case it is 60/8. We can readily believe that all of nature's laws in general, and the velocity of light in particular, should be the same on sun, earth or planet in the Milky Way; but the fact that the earth-man finds the sun clocks slow, and the sun-man finds the earth clocks slow, in the same ratio is the startling contribution of the theory of relativity: that two actions simultaneous upon one system should not be simultaneous when viewed from another system is surprising.

Let us see what the two observers on the sun have to tell us about one of the clocks on the earth.

A and B are sixty miles apart and can not both see the earth clock at the same time; but the earth is a moving system, hence A can compare his clock with the earth clock, and later B can make a similar comparison.

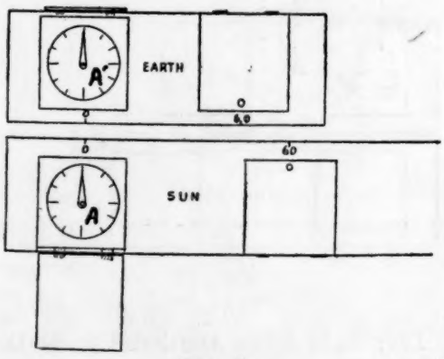

FIG. 6

When the earth clock is opposite A (Fig. 6), the latter finds the hand of his clock at 12, and the hand of the earth clock at 12; as the earth moves on, the earth clock of A^1 will reach a position opposite B (Fig. 7), who finds that the hand of his clock has again reached 12, while the hand of the earth clock has reached only 9; hence A and B establish the fact that twelve hours on the sun are equal to nine hours on the earth; that is, the earth clock runs slow in the ratio of 3 to 4.

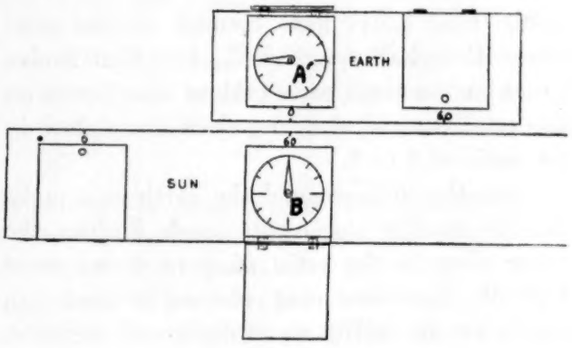

FIG. 7

When the earth-men agree to make observations on one of the sun clocks, they reach a similar conclusion. A^1 and B^1 are sixty miles apart and can not both see the sun clock at the same time; when B^1 comes to a position opposite the sun clock (Fig. 8), the hand of

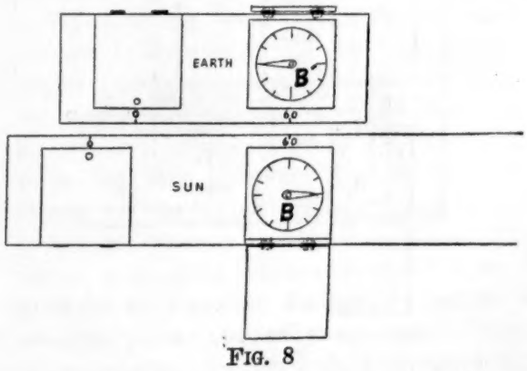

FIG. 8

his clock is at 9, while the hand of the sun clock is at 3; when, as the earth moves on, A^1 comes opposite the sun clock (Fig. 9), he finds that twelve hours have elapsed, for the hand of his clock is again at 9, but the hand of the sun clock has only gone from 3 to 12; in other

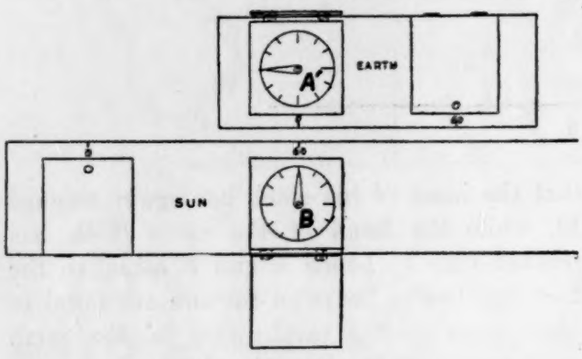

Fig. 9

words, nine hours have elapsed on the sun; hence A^1 and B^1 establish the fact that twelve hours on the earth are equal to nine hours on the sun; that is, the sun clock runs slow in the ratio of 3 to 4.

Since the sun-men and the earth-men make exactly similar statements, each finding the other slow in the ratio of 3 to 4, we must logically conclude that the earth and sun clocks are in reality equivalent, and establish the same unit of time.

The standard of length upon the sun seems, moreover, to be different from that used upon the earth (Fig. 10), but that too is true from

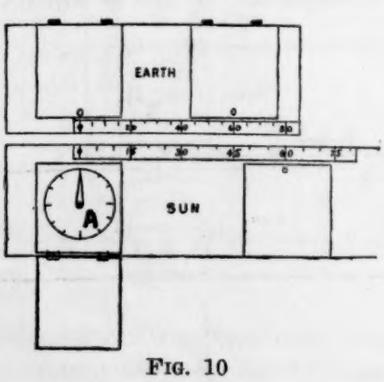

Fig. 10

one view-point (one coordinate system) only; the standards of length as well as of time are in reality equivalent, as we will now proceed to demonstrate.

Let us remember that simultaneity is established only by the clocks.

The sun-men wish to compare their length standard with the standard used upon the earth. The lengths they wish to compare are such that two observers are necessary, one at each end of the scale. A and B decide to compare their scale reading with the scale reading opposite them at the same moment of time. At 12 o'clock A (Fig. 10) finds zero of his scale opposite zero on the earth's scale; B watching his clock finds, when the hand is at 12 (Fig. 11), that 60 on his scale is oppo-

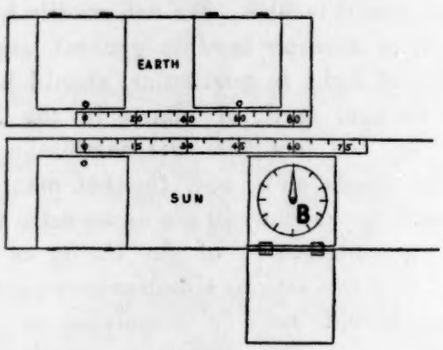

Fig. 11

site 80 on the earth's scale. Hence A and B conclude that 60 sun miles are equal to 80 earth miles, or that the earth mile is shorter in the ratio of 3 to 4.

The earth-men, by similar observations, compare their length standard with the standard upon the sun. A^1, watching his clock

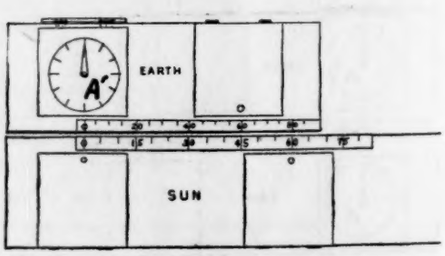

Fig. 12

(Fig. 12), finds when the hand is at 12, that zero on his scale is opposite zero on the sun scale. B^1 finds, when the hand of his clock is at 12 (Fig. 13), that 60 on his earth scale is opposite 80 on the sun scale. Hence they reason that 60 earth miles are equal to 80 sun

miles; or that the sun miles are shorter in the ratio of 3 to 4.

This apparent paradox is due to the fact that we are not accustomed to establishing simultaneity by accurate instruments of time,

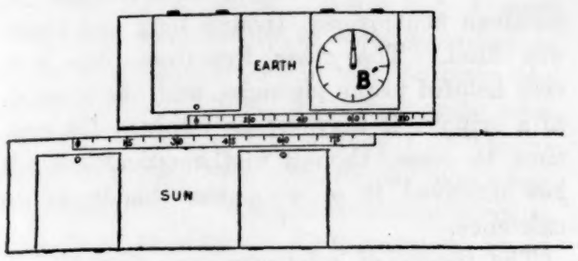

FIG. 13

but rather by a vague "now" which can be established neither by clear thought nor by experiment. We have been thinking absolute time which can not be measured, hence is meaningless; the only time that has meaning is the time we can measure with nature's instruments of time, her uniform processes.

Let us study for a moment the clocks on the moving system; to an observer outside the moving system, the two clocks will be out of synchronism; viewed from the sun (Fig. 14),

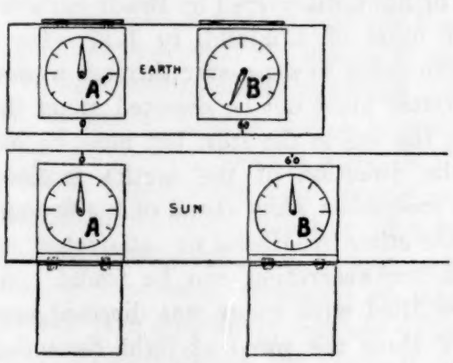

FIG. 14

B''s clock will be five and one third hours behind the clock of A'. If, however, A''s clock be moved to B''s station, it will no longer be five and one third hours ahead of B''s clock, but will record the time of that station in perfect agreement with B''s. In the process of moving from its own station at zero to B''s station at 60, A''s clock must therefore have gradually slowed up. *Vice versa,* if B''s clock be moved to A''s station, it will on arrival no longer be five and one third hours behind A''s clock, but in agreement with it; in moving against the direction of the moving system it has gained five and one third hours in time.

Since the clocks are all nature's timepieces, all the clocks in one system (and we can imagine an infinite number of them) move in perfect uniformity. Each point or station on the system has its own particular local or place time (Eigenzeit). If a clock be moved from one station to another, on reaching its new station it records the place-time of that station. It would seem therefore that the impulse which sets the clock in motion in the direction of the moving system acts upon the balance wheel,—or whatever may be the clock regulator, to retard it; and the impulse which sets the clock in motion counter to the moving system acts upon the balance wheel to accelerate it.

The logical deductions that follow from these facts are so startling to the lay mind that I prefer to translate from Einstein himself:[6]

> Give the watch a very large velocity (approximating the velocity of light) so that it travels with uniform speed; after it has gone a long distance give it an impulse in the opposite direction so that it returns to its starting point. We then observe that the hand of this watch during its entire journey to and fro has remained practically at a standstill, while the hand of an exactly similar watch which did not move with respect to the coordinate system (the sun or earth) has changed its position considerably.
>
> We must add: what is true for our watch with respect to time must also be true of any other enclosed physical system, whatever its nature, because in all our thinking the watch was introduced simply as a representative of all physical actions or occurrences. Thus, for example, we could substitute for the watch a living organism enclosed in a box. Were it hurled through space like the watch, it would be possible for the organism, after a flight of whatever distance, to return to its starting point practically unchanged, while an exactly similar organism which remained motion-

[6] Zurich, *Vierteljahresschrift d. Natf. Gesell.,* 56: 1–230, 1911. Reprinted in Berlin; *Naturw. Rundshau,* 28: 285, 1912.

less at the starting point might have given place to new generations. For the organism in motion time was but a moment, if its speed approached the velocity of light. This is a necessary consequence of our fundamental assumptions and one which experience imposes on us.

Let us return to the experiment of Michelson and Morley with which we started. Let us interpret it by means of our model. We have spoken of the sun and earth in uniform translation through space; let us symbolize this by the moving system of our model, the clock at zero being the sun, and the clock at 60 representing the earth; let us send a light flash in the direction of their common translation; it starts from the sun, A^1, at 12 o'clock and reaches the earth, B^1, at 8 o'clock (Fig. 5), thus 8 units of time have elapsed. If we send the light flash against the sun and earth translation, then B^1 becomes the sun and A^1 the earth; the light flash leaves the sun, B^1, at 8 o'clock and again reaches the earth, A^1, at 4 o'clock, 8 units of time having elapsed, exactly as is the case when sun and earth are at rest (Fig. 3).

The assumptions of the principle of relativity are:

1. That among all fixed star systems not one is unique—that as far as physical phenomena are concerned it is immaterial upon what system of reference we base measurement.

2. When a light-pulse or particle travels through empty space the ratio of distance traversed to the time taken to go that distance, both measured in *any* physical system whether considered at rest or in translation, is invariant.

These two assumptions are interpretations of experimental facts, and the conclusions deduced from them as given in this paper cannot be invalidated unless these primary assumptions are shown to be misinterpretations of experiment.

To what conclusion in respect to an interstellar ether[7] does the principle of relativity lead us? That there is no place for the ether hypothesis. If the latter were correct, the ether would possess uniqueness which the first assumption of relativity denies to all bodies occupying space. Primitive man endowed our earth with uniqueness, but the Copernican controversy, though long and bitter, was final. The ether hypothesis has been very helpful to the physicist, and like a crutch to a cripple, it may yet be retained for some time to come, though mathematical analysis has deprived it of even the shadow of an existence.

The theory of relativity says that Michelson's experiment, far from being negative as Michelson thought, was exactly what was to be expected. How could an ether drift be established when the ether had no physical existence?

Relativity theorists are reconsidering Newton's suggestion as to the corpuscular structure of light and a new theory of radiation based upon the idea of quanta [discrete physical energy elements] is now being worked out in Germany and Holland. Newton's theory gave us an easy explanation of the aberration of light, discovered by Bradley, the astronomer royal of England, in 1727. He found that in order to see a star through a telescope, the latter must not be directed along the line from the eye to the star, but must be inclined in the direction of the earth's motion, just as a sportsman aims ahead of his fleeing prey. On the ether hypothesis no satisfactory explanation for aberration can be found.[8] A telescope filled with water was directed toward a star:[9] since the speed of light through water is three-fourths of the speed of light through air, a large variation in the angle of aberration was expected; but the variation found was far from what theory had predicted.

[7] H. A. Lorentz, *Physikalische Zeitschrift*, 11: 1234, 1910; Max Planck, p. 110, "Acht Vorlesungem" (Columbia Lectures, 1910); Geo. B. Pegram, *Educational Review*, 41: 290, 1911; Albert P. Carmon, *School Science and Mathematics*, 13: 1, 1913; M. Laue, *Physik. Zeitschrift*, 13: 118, 1912; Norman Campbell, *Physik. Zeitschrift*, 13: 120, 1912.

[8] H. A. Lorentz, "Electrische Erscheinungen," 1906, p. 1.

[9] Airy, *Proc. Roy. Soc. London*, 20: 35, 1871; 21: 121, 1873; *Phil. Mag.*, 43: 310, 1872.

Poincaré and Favé, Lord Rayleigh,[10] and Brace,[11] each hoped to find effects of the earth's translation through the ether in the double refraction of light by crystals, but were unable to obtain such effects. The phenomena of interference upon which Young and Fresnel based their wave theory of light have not as yet been completely accounted for upon the principle of relativity. Spectroscopists seem to prefer the use of "frequency" to "wave-length" in their descriptions of monochromatic radiation.[12] When any action forcibly ejects from an atom a light particle or an electron spinning in its atomic orbit with the speed of light, it is not difficult to perceive that the ejected light-particle would have a circular motion superimposed upon its apparent translation through interstellar space. Thus the path of a light particle would be spiral or screw-shaped, wave-length would correspond to the pitch of the screw, and frequency to the number of revolutions which the light particle makes per second. Possibly a theory of interference may be worked out along this line.

The new mechanics teaches that the velocity of light, 186,337 miles per second, is our limit of speed; no body in motion can exceed it, and can only with extreme difficulty approach it.

The old mechanics taught that a constant force continually acting upon a body in interstellar space, would make it go faster and faster without limit. The principle of relativity says that a constant force acting upon a body during successive intervals of time meets a greater and greater opposition to its increasing speed; and when it has attained the speed of light the power of this force to produce acceleration is exhausted. There is nothing, of course, in empty interstellar space to prevent a body from continuing forever with the speed once acquired, except collision with another heavenly body, when perchance, the energy of motion changes the cold colliding matter into a radiant sun.

[10] Rayleigh, *Phil. Mag.*, 4: 683, 1902.

[11] Brace, *Phil. Mag.*, 7: 328, 1904.

[12] Runge, *Zeitschrift f. Electrochemie*, 18: 485, 1912.

The old mechanics was quite sufficient until the discovery of cathode rays and radium gave us matter in motion outspeeding a thousand times our fastest planet, Mercury. The particles in the stream of cathode rays travel with a velocity of 5,000 miles a second, while those from radium are hurled into space with a speed of 50,000 to 178,000[13] miles per second, and have become in many instances the modern surgeon's scalpel. The physicist, in his search for law with respect to matter, finds it necessary to readapt his mechanics to modern facts.

The question naturally arises: why should the speed of light be set as a limit to the increase of velocity? The answer is that with unlimited speed the possibility arises of a reversal in the order of time. This possibility has been worked out in a curious way by Flammarion.[14] He makes Lumen, an interstellar traveler, an observer of the battle of Waterloo, and then proceeds to show what would happen if, at the close of the battle, he were moving away from the scene with a velocity greater than the speed of light; he would overtake the light which left the battlefield at the beginning of the engagement, and would see the whole fray in a reversed order of time, like a moving picture film run off backward.

Secondly, if Lumen were at rest and the earth were speeding away from him with a velocity greater than that of light, he would see the battle in its natural order, but all would proceed with stately slowness.

Thirdly, if Lumen were at rest and the earth were speeding toward him with a velocity greater than the speed of light, he would again see the battle in the reverse order, as the last fire from the guns would come to Lumen from a point nearer to him than the light from the first volley.

With any experiment thus repeated three times, Lumen would be able to determine whether he were at rest and the earth in motion, or *vice versa;* in other words, he would

[13] Rutherford, *Physikalische Zeitschrift*, 13: 1178, 1912.

[14] C. Flammarion, "Stories of Infinity—Lumen," p. 74, 1873.

be able to establish absolute motion, a contradiction of the first assumption of the principle of relativity.

Hence in all physical problems where there is a possibility of two solutions, the one which leads to the establishment of an absolute velocity must be rejected, and the alternative solution accepted as valid.

The principle of relativity, besides clearing our minds of the cobwebs of absolute time and space, gives us, through its explanation of physical experiments, a deeper consciousness of the manifoldness of space, in which time is, not the flow of duration suggested by the immortal Newton, but any one of the spacial manifolds so beautifully developed by Heinrich Minkowski in his "Raum und Zeit," and by Wilson and Lewis in the *Proceedings of the American Academy* for 1912.

Reinhard A. Wetzel
The College of the City of New York

GRANTS BY THE BRITISH ASSOCIATION

At the Birmingham meeting of the British Association for the Advancement of Science grants in aid of scientific research amounting to about $6,000 were made as follows:

Mathematical and Physical Science: Profesor H. H. Turner, seismological observations, £60; Dr. W. N. Shaw, upper atmosphere, £25; Sir W. Ramsay, constants and numerical data, £40; Professor M. J. M. Hill, calculation of mathematical tables, £20; Lieut.-Col. A. Cunningham, copies of the "Binary Canon" for presentation, £5.

Chemistry: Dr. W. H. Perkin, study of hydroaromatic substances, £15; Professor H. E. Armstrong, dynamic isomerism, £25; Professor F. S. Kipping, transformation of aromatic nitroamines, £15; A. D. Hall, plant enzymes, £25; Professor W. J. Pope, correlation of crystalline form with molecular structure, £25; Professor H. E. Armstrong, solubility phenomena, £15.

Geology: R. H. Tiddeman, erratic blocks, £5; Professor P. F. Kendall, list of characteristic fossils, £5; Dr. A. Strahan, Ramsay Island, Pembroke, £10; Professor Grenville Cole, old red sandstone of Kiltorcan, £10; G. Barrow, trias of western midlands, £10; Professor W. W. Watts, sections in Lower Paleozoic rocks, £15.

Zoology: Dr. A. E. Shipley, Belmullet Whaling Station, £20; Dr. Chalmers Mitchell, nomenclator animalium, £50; S. F. Harmer, Antarctic whaling industry, £90.

Geography: Professor J. L. Myres, maps for school and university use, £40; Professor H. N. Dickson, tidal currents in Moray and adjacent firths, £40.

Engineering: Sir W. H. Preece, gaseous explosions, £50; Professor J. Perry, stress distributions, £50.

Anthropology: Dr. R. Munro, Glastonbury Lake Village, £20; Sir C. H. Read, age of stone circles, £20; Dr. R. Munro, artificial islands in Highland lochs, £5; Professor G. Elliot Smith, physical character of ancient Egyptians, £34; Professor J. L. Myres, anthropometric investigations in Cyprus, £50; Professor W. Ridgeway, Roman sites in Britain, £20; Dr. R. R. Marett, Paleolithic site in Jersey, £50.

Physiology: Professor E. A. Schäfer, the ductless glands, £35; Professor A. D. Waller, anesthetics, £20; Professor J. S. Macdonald, calorimetric observations, £40; Professor C. S. Sherrington, mammalian heart, £30.

Botany: Professor F. J. Oliver, structure of fossil plants, £15; Professor A. C. Seward, Jurassic flora of Yorkshire, £5; Professor F. Keeble, flora of peat of Kennet Valley, £15; A. G. Tansley, vegetation of Ditchan Park, £20; Professor F. F. Blackman, physiology of heredity, £30; Professor F. O. Bower, renting of Cinchona Botanic Station in Jamaica, £25; Professor W. Bateson, breeding experiments with *Œnotheras*, £20.

Education: Professor J. J. Findlay, mental and physical factors, £30; Dr. G. A. Auden, influence of school books on eye-sight, £15; Sir H. Miers, number, etc., of scholarships, held by university students, £5; Dr. C. S. Myers, binocular combination of kinematograph pictures, £10; Professor J. A. Green, character and maintenance of museums, £10.

SCIENTIFIC NOTES AND NEWS

The British Association for the Advancement of Science has accepted an invitation to hold the meeting of 1915 at Manchester. It will be remembered that next year's meeting will be held in Australia under the presidency of Dr. William Bateson.

There have been called to the Research Institute for Biology, established under the

Kaiser Wilhelm Society, Dr. Goldschmidt, of Munich, known for his experiments on Mendelian heredity in animals; Dr. Hartmann, of the Berlin Institute for Infectious Diseases, known for his work on protozoa, and Dr. Warburg, son of the director of the Reichsanstalt, who will have charge of work on cell physiology. It was noted last week that Dr. Carl Correns will be director of the institute.

DR. DAVID HILBERT and Dr. Felix Klein, professors of mathematics at Göttingen, have been elected corresponding members of the Berlin Academy of Sciences.

DR. MAX PLANCK, professor of mathematics, has been elected rector of the University of Berlin.

COUNTESS PROSKOWIA UWAROW, of Moscow, known for her work in archeology, has been given an honorary doctorate by the University of Königsberg.

DR. WILHELM ALEXANDER FREUND, the distinguished German gynecologist, has celebrated his eightieth birthday.

M. EMIL BOUTROUX, of Paris, and Professor Alois Riehl, of Berlin, both distinguished for their contributions to philosophy, will make addresses at the opening of the graduate school of Princeton University.

DR. W. F. G. SWANN, demonstrator in physics in the University of Sheffield, has been appointed physicist in the laboratory of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington.

PROFESSOR C. W. THOMPSON, chief of the bureau of research in agricultural economics at the University of Minnesota, has taken charge of work in the rural organization service of the U. S. Department of Agriculture.

DR. HENRY CARTER ADAMS, professor of political economy at the University of Michigan, has accepted the post of general fiscal adviser to the Republic of China.

MAJOR B. K. ASHFORD has been appointed president of a board for the study of tropical diseases in Porto Rico under the medical department of the army.

THE *Annalen der Naturphilosophie* will hereafter be named the *Annalen der Natur- und Kulturphilosophie*. Professor R. Goldscheid will be associated with Professor Ostwald in editing the periodical.

AT Princeton University Professor Henry B. Fine has returned from a year's leave of absence in Europe and resumed his duties as head of the department of mathematics and dean of the department of science. Professor George A. Hulett, who was last year acting as chief of the department of chemistry in the United States Bureau of Mines, has resumed his professorship of physical chemistry. The members of the faculty on leave of absence this year include: Professor Norman Kemp Smith, head of the department of philosophy (first term); Professor Augustus Trowbridge, of the department of physics, and Professor Oswald Veblen, of the department of mathematics.

PROFESSOR M. A. CARLETON, cerealist of the U. S. Department of Agriculture, has recently resumed his duties in that department, after a year and three months' leave of absence as general manager of the Pennsylvania Chestnut Tree Blight Commission.

PROFESSOR A. E. KENNELLY, of Harvard University, represented the U. S. Committee and the U. S. Bureau of Standards at the International Illumination Commission in Berlin, August 26–30, and at the International Electrotechnical Commission, Berlin, September 1–5.

PROFESSOR AUGUSTUS D. WALLER, M.D., F.R.S., of the University of London, will lecture before the Harvey Society at the New York Academy of Medicine, at 8.30 P.M., October 4, 1913, on "A Short Account of the Origin and Scope of Electrocardiography." Professor Waller brings from London his own apparatus especially for this lecture and will give a series of demonstrations. The lecture is open to the public.

A COURSE of three lectures dealing with the early history of medicine will be given before the Royal Society of Medicine, London. The first lecture will be on October 10, by Professor

Morris Jastrow, of the University of Pennsylvania, and will treat of Babylonian medicine; the subsequent lectures will be by Professor Elliot Smith, on Egyptian medicine, and by Professor R. Caton, on Greek medicine.

Professor von Baelz, for thirty years professor of medicine in the University of Tokyo, the author of contributions to medicine and anthropology, has died at Stuttgart, aged sixty-four years.

Professor John Robie Eastman, professor of mathematics in the navy from 1865 to 1898, when he was retired for age, died on September 26, at the age of seventy-seven years. In 1906 Professor Eastman was promoted to the rank of rear admiral in the navy. He had made distinguished contributions to solar, stellar and meteoric and planetary astronomy.

Dr. John Green Curtis, from 1876 to 1909 professor of physiology in the College of Physicians and Surgeons, Columbia University, and since emeritus professor, died on September 20, aged sixty-nine years.

Dr. Charles Lester Leonard, professor of roentgenology in the University of Pennsylvania, died on September 23, aged fifty-two years, from X-ray dermatitis, contracted in the course of his work nine years ago.

Dr. Arnold Rossel, formerly professor of chemistry at Bern, has died at the age of sixty-eight years.

Professor Paul Adolf Näcke, director of the insane asylum at Colditz, known for his contributions to psychiatry, has died at the age of sixty-three years.

The death is also announced of Dr. Georg Roth, emeritus professor of mathematics at Strassburg.

The Washington Biologists Field Club has passed the following resolution:

In the death of Edward Lyman Morris, one of the founders of the Washington Biologists Field Club, on September 14, 1913, at Brooklyn, N. Y., this association has lost a member whose deep interest in its affairs never failed from the first days of organization to the last moments of his life. Although duty called him to another city, he never lost an opportunity to advance the interests of the club, and on the week preceding his death he spent three days at his beloved island and recorded on the register the flowering of a rare plant.

The members of this club mourn the loss of an ardent worker, a congenial companion, a respected associate and friend.

Resolved, that the Washington Biologists Field Club extend to the family of our deceased member its sincerest sympathy and condolence.

For the Club,

E. A. Schwarz,
A. K. Fisher,
H. C. Fuller

The U. S. Civil Service Commission announces an examination for associate physicist in theoretical and experimental optical research to fill a vacancy in this position in the bureau of standards, Department of Commerce, Washington, D. C., at a salary of $2,500 a year.

Examinations will also be held for quarry technologist to fill a vacancy in the Bureau of Mines at Washington, D. C., at a salary ranging from $2,500 to $3,000, and for junior physicist in the Bureau of Mines, Pittsburgh, Pa., and other places as they may occur, at a salary ranging from $1,020 to $1,200 a year.

The seventh annual convention of the National Society for the Promotion of Industrial Education and the organization meeting of the National Educational Guidance Association will be held at Grand Rapids, Mich., from October 19 to 25.

The annual meeting of the American Institute of Chemical Engineers will be held in New York from December 10 to 13.

Under the auspices of the school of mines at Berlin, there are offered prizes amounting to 2,000 marks for promoting safety in mines.

Arrangements are being made for an expedition to King Edward the Seventh's Land, a tract stretching from the Great Ice Barrier, to start in August next year. It will be under the command of Mr. J. Foster Stackhouse, who was intimately associated with Captain Scott in organizing the voyage of the *Terra Nova*. It is proposed that the members of the expedi-

tion sail from the Thames about the middle of August, 1914, in the steam yacht *Polaris*, a ship especially built in Norway for ice navigation in accordance with designs approved by an international committee of explorers, including Charcot, de Gerlache and Nansen. The expedition will, it is expected, be away for 20 months or more.

MR. TRUMAN H. ALDRICH, of Birmingham, Ala., has presented to the Museum of the Geological Survey of Alabama his entire conchological collection, by estimate about 20,000 species from all parts of the world. In addition to his own extensive gatherings and exchanges during more than 50 years, Mr. Aldrich had purchased largely not only from dealers but from such special workers as Garrett and Doherty. He had bought outright several important private collections, notably the entire Mauritius gatherings of the late Col. Nicholas Pike, the Jones Bermuda and Nova Scotia Shells, and the Parker cabinet of about 5,500 listed species. The Aldrich collection is particularly rich in Asiatic and Indian forms. The series of operculate land shells could hardly be matched in this country, and there are many types of species described by Mr. Aldrich and others. With the shells were given 1,300 or more volumes of conchological and other scientific works. Mr. Aldrich had already given all his duplicates, probably 200,000 specimens, to the museum, and last year he donated a very large and fine series of Tertiary invertebrate fossils. The museum, it may be noted, moved into its new building, Smith Hall, less than four years ago. Though the outcome of the Geological Survey and bearing its name, it is by law an integral part of the University of Alabama. Dr. Eugene A. Smith, since 1873 at the head of the survey, is also director of the museum.

DR. W. A. SAWYER, director of the hygienic laboratory of the California State Board of Health, and W. B. Herms, assistant professor of parasitology in the University of California, have contributed to the *Journal* of the American Medical Association an article in which they reach the following conclusions: (1) In a series of seven experiments in which the conditions were varied, we were unable to transmit poliomyelitis from monkey to monkey through the agency of the stable-fly. (2) Further experimentation may reveal conditions under which the stable-fly can readily transfer poliomyelitis, but the negative results of our work and of the second set of experiments of Anderson and Frost lead us to doubt that the fly is the usual agent in spreading the disease in nature. (3) On the basis of the evidence now at hand we should continue to isolate persons sick with poliomyelitis or convalescent, and we should attempt to limit the formation of human carriers and to detect and control them. Screening of sick-rooms against the stable-fly and other flying insects is a precaution which should be added to those directed against contact infection, but not substituted for them. (4) The measures used in suppressing the house-fly are not applicable to the control of the stable-fly owing to its different breeding habits and food-supply. Methods should be devised for diminishing the numbers of stable-flies, as they are a great annoyance to cattle and, in all probability, are capable of transferring and inoculating a number of the diseases of animals.

THE birth of ten calves in the buffalo herd maintained by the government on the Wichita national forest and game refuge, near Lawton, Oklahoma, has been reported by the game warden in charge. The herd now contains a total of 48 head of full-blooded buffalo, or, more properly, bison, of which 27 are males and 21 females. All of the animals are in good condition. In 1907 the American Bison Society donated to the federal government a nucleus herd of 15 animals which had been bred and reared in the New York Zoological Park. The animals were transported to the Wichita national forest, which is also a game refuge, and placed under the care of the Forest Service. They readily adapted themselves to their new habitat, but the area upon which they were placed was within the zone affected by the Texas fever tick and during the two or three years following their transfer only the constant care and watchfulness of the forest

officers prevented the complete loss of the herd. The animals were examined almost daily to determine whether they had become infested with Texas fever ticks and were placed in specially designed cages and sprayed with crude oil at intervals of from fifteen to thirty days, but notwithstanding the extreme precautions which were adopted, three of the animals died. Gradually, however, the enclosures in which the buffalo were confined were freed from fever ticks and there is a possibility that as the buffalo adapted themselves to their new environment they became more or less immune to the disease. No losses from Texas fever have occurred for several years, and the herd has almost quadrupled in number since it was established. The fact that the herd has not increased more rapidly is due largely to the preponderance of male calves. This characteristic of the buffalo is so pronounced in all of the herds now in captivity that a cow is considered twice as valuable as a bull.

UNIVERSITY AND EDUCATIONAL NEWS

Ernest Solvay, the discoverer of a process for the manufacture of soda, celebrated the fiftieth anniversary of that discovery on September 2 at Brussels by giving more than $1,000,000 to educational and charitable institutions and the employees of his firm. The Universities of Paris and Nancy each received $100,000.

At the last session of the Legislature of Pennsylvania an appropriation of $40,000 was made to aid in the development of courses in education at the University of Pennsylvania. Dr. Frank P. Graves, of the Ohio State University, has been appointed professor of the history of education, and Dr. Harlan Updegraff, of the Iowa State University, as professor of educational administration. Professor A. Duncan Yocum, who now occupies the chair of pedagogy at the University of Pennsylvania, will continue as professor of educational research and practise.

A graduate school of education has been established at Bryn Mawr College. It is under the charge of Professor Kate Gordon, associate professor of education, Dr. Matilde Castro, director of the Model School, and Professor James H. Leuba, professor of psychology, who will give a graduate course on the psychology of defective and unusual children.

The University of California has announced the establishment of a new Division of Rural Institutions. This new department will study and aid the rural forces which have for their aim the making of life in the open country successful and satisfactory. Elwood Mead has been called to the headship of this new division. He was formerly chief of the United States Bureau of Irrigation Investigations. He is now in Australia, as chairman of the Rivers and Water Supply Commission of the State of Victoria and chief engineer. His work in the University of California will be to deal with questions of farm credits, irrigation and drainage institutions, cooperation, and all the varied political, economic, educational, social and religious institutions which affect rural life.

Work has been begun at Smith College on the erection of a new biological laboratory which is to cost $150,000.

Professor Don Rosco Joseph, of Bryn Mawr College, has accepted a call to the medical school in St. Louis. His work in physiology at Bryn Mawr College will be given by Professor Arthur Russell Moore, now assistant professor in the University of California.

Dr. Paul S. McKibben has left the department of anatomy of the University of Chicago to become professor of anatomy in the Western University of London, Ontario.

Dr. G. E. Coghill, of Denison University, has been appointed associate professor of anatomy at the University of Kansas, Lawrence.

Edmund Vincent Cowdry, associate in anatomy of the University of Chicago, goes this fall to the Johns Hopkins Medical School.

Dr. Clara Moore, pathologist in the North Chicago Hospital, has been appointed instructor in clinical medicine and diagnosis in the University of Wisconsin.

DR. HENRY E. RADASCH, assistant professor of histology and embryology at Jefferson Medical College, has been appointed instructor of anatomy in the Pennsylvania Academy of Fine Arts to succeed the late Dr. George B. McClellan.

DR. C. C. LIPP, assistant professor of veterinary science at the University of Minnesota, has been elected head of the department of veterinary science of the South Dakota Agricultural College.

AT Norwich University Dr. S. F. Howard, formerly associate professor at Amherst College, is to be head of the chemistry department. J. E. Lear, B.S., formerly associate professor in the Texas College, Texas, has been appointed assistant professor of physics and mathematics.

AT the University of Pennsylvania Dr. Thomas D. Cope and Dr. E. A. Eckhardt have been promoted to assistant professorships in physics; Dr. Walter T. Taggart to the grade of professor of organic chemistry; Dr. Owen L. Shinn to be professor of applied chemistry, and Dr. Herman C. Berry to be professor of materials of construction.

HARRY WALDO NORRIS, A.M., professor of zoology at Grinnell College, has been appointed to give instruction in zoology in Harvard University during the year 1913–14, in accordance with the agreement with western colleges. His term of service will fall in the second half-year.

DR. M. BARTUZZI has been appointed to a newly established chair of medical history in the University of Siena.

DISCUSSION AND CORRESPONDENCE

THE BREAD SUPPLY

IN SCIENCE of August 22, 1913, appear twenty columns of words from Professor H. L. Bolley, entitled "Cereal Cropping: Sanitation, a New Basis for Crop Rotation, Manuring, Tillage and Seed Selection." Under this imposing and comprehensive title we find that eighteen columns are devoted chiefly to belittling the work of chemists, agronomists, bacteriologists, and also agricultural advisers who accept the findings of such scientists. Occasionally Professor Bolley hedges with the assertion that he knows plant food to be essential, and then renews the attack in such words as these:

> On account of all these conditions of low yield and invariable deficiency in quality, there has gone up a great cry of "depleted" soils, "worn out" land, "bad agriculture," "shiftless methods," etc. This cry follows the plowman regardless of his improved tools and general farming improvements, regardless of better methods of tillage which we know now obtain on the farm, as against those which our forefathers were able to accomplish, and all regardless of hard work. It is all right for the banker and the lawyer, and even some professors, to berate the farmer for idleness and inefficiency in methods and lack of business, but I say let such men try to raise wheat of high grade under the present general understanding as laid down in books, or by our best agriculturists. In spite of all these directions, the wheat soon becomes soft and shows all of the peculiar characteristics which we find named in the literature of the chemical laboratory, or in the milling tests of wheat as previously indicated, "white-bellied," "piebald," or shrivelled, bleached and blistered, "black-pointed," in fact all of the qualities of deteriorated grain; and the chemist from his laboratory outlook cries out "depleted soils," "lost fertility," "bad physical texture," due to "worn-out humus," "lost nitrogen," "insufficient phosphates," "lime," etc., forgetting, as it were, that almost every field in these matters is a law unto itself and that every one of these fields in the next few years may contradict all these assertions by the growth of splendid crops for reasons no one seems to know. The expert agriculturist and agronomist, who take their cue largely from the chemists, cry out: "Give us intensified agriculture," "Apply phosphates," "Apply lime," "Apply potash," "Grow clover," "Raise corn," "Rotate," all in a confused jumble, and lately the bankers, afraid of their mortgages, have become very busy and tell how to farm and scold rather strongly about lack of business methods on the farm, berate the schools, etc.
>
> These conditions of farm cropping, though not exclusively American, are especially in prominence at present because many of our most noted publicists are becoming, perhaps properly, alarmed. They say our farmers show no ability of maintaining the supply of wheat, the bread grain, a

permanent cropping element of old land agriculture, but rather, instead, are reaping lessened yields of poorer quality from larger acreages.

In columns one and twenty the "new basis" is revealed:

> Deteriorated wheat, as seen in depressed yields and low quality, as now quite commonly produced in the great natural wheat-producing regions of this country, is not, primarily, a matter of lost fertility or of modified chemical content of the soil, but is specifically a problem of infectious disease which is superimposed upon the problems of soil and crop management.
>
> My experience with cereal crops with reference to the application of fertilizers, the trial of varieties, experiments in seed selection, seed breeding and seed treatment, and seed purification furnish data which will allow me to say that I have no fear that all will eventually agree that sanitary considerations with reference to the characteristics of parasitic diseases which are now quite commonly resident in the seed and the soil will yet form the essential basis for the proper management of crops in rotation in series, and the same considerations will largely govern the type of tillage and the manner of handling waste materials on the farm, particularly farm manures.

Professor Bolley heartily commends himself for "having grown up on the farm, and never having allowed himself to get away from the real love of working in the dirt"; but he fears that "too many of our workers who are paid to investigate agricultural problems may only investigate for their own enjoyment—may again deal in formulas, and theories, books and philosophies, and thus give out to the working public fine philosophies which may yet leave the worker helplessly in the dark as to what to do."

As an average of sixty years where wheat has been grown year after year on the same land at Rothamsted, the unfertilized land produced 12.6 bushels per acre, while 35.4 and 37.0 bushels were the respective yields where farm manure and commercial plant food were applied.

As an average of twenty-four years the wheat yields at Pennsylvania State College varied from 10.1 bushels on unfertilized land to 24.1 with farm manure and 24.8 with commercial plant food, when grown in a four-year rotation.

As an average of nineteen years the wheat yields at the Ohio Experiment Station were 10.2 bushels on unfertilized land, 21.7 bushels with farm manure, and 26.9 bushels where commercial plant food was applied, the wheat being grown in a five-year rotation with clover, timothy, corn and oats, on five different series of plots, so that every crop might be represented every year.

As compared with Rothamsted, Pennsylvania and Ohio, the more extensive and very practical field experiments now being conducted by the University of Illinois in many different parts of the state are new and inconclusive, but the results secured in 1913 from fields that have been in operation twelve years (see the accompanying tabular statement) not only represent much field work, but they also support the equally valuable analytical data from the chemical laboratory involving analyses of thousands of representative soil samples collected in connection with the detail soil survey of more than forty counties.

1913 Wheat Yields: Bushels per Acre
From University of Illinois Experiment Fields

Location of Experiment Field	Number of Years in Crop Rotation	Soil Fertilization				
		None	Organic Manures	Organic Manures, Limestone	Organic Manures, Limestone, Phosphorus	Organic Manures, Limestone, Phosphorus, Potassium
Urbana....	Four	11.1	13.6	19.7	34.6	31.7
Odin.....	Four	17.3	20.4	29.8	36.0	37.0
DuBois.....	Four	7.2	no test	18.1	33.4	29.9
Cutler.....	Three	8.8	7.0	19.5	31.0	30.7
Mascoutah	Four	21.0	22.1	24.0	32.9	32.3
General average...		13.1	—	22.2	33.6	32.3
Average increase..		—	—	9.1	20.5	19.2

The unfertilized surface soil of these fields contains in two million pounds (corresponding to an acre of land about 6⅔ inches deep) from 700 to 1,200 pounds of total phosphorus and from 25,000 to 36,000 pounds of total potassium. Where organic manures are pro-

vided for supplying nitrogen and liberating mineral plant food in rational systems of farming, the relationship between the chemical composition of the soil and crop production is normally very apparent. Irrational systems often give abnormal results, and their correct interpretation requires that no important factor of influence shall be ignored.

It may be added that the wheat from our well-treated and high-yielding plots is not of poor quality, but of very high grade, and has been sold to the experienced grain buyer at a premium as high as 15 cents per bushel above that paid for wheat from unfertilized well-rotated land.

In Illinois, as in all other states, most of the soil and crop investigators are men of large practical farm experience, but we also have deep respect for the science of analytical chemistry, as the only means of determining the total stock of plant food in the soil, and for the science of biochemistry, as the chief means of making plant food available.

Chemists and agronomists must honor Jensen for the information and method which he gave to the world relating to the destruction of fungous diseases sometimes carried in seed grain, and we honor Bolley for his valuable contributions in this field of agricultural research; but we also recognize that the avoidance of fungous diseases as one among the many advantages and reasons for crop rotation and for the proper handling of crop residues is not a new idea, for it has been advanced, explained and emphasized by numerous investigators for many years. The persistent efforts to belittle the importance of positive soil enrichment and preservation in permanent rational systems of farming, whether by improvident landowners, by Whitney and Cameron, of the Bureau of Soils, or by Professor Bolley, are the greatest curse to American agriculture and the greatest danger to permanent prosperity in this country.

The fact that the earth is round became generally accepted two or three centuries after its discovery; and it required a full century for Europe to half appreciate the great discovery by De Saussure, so well expressed in the words of Liebig:

> It is not the land itself that constitutes the farmer's wealth, but it is in the constituents of the soil, which serve for the nutrition of plants, that this wealth truly consists.

The foundation principles for the restoration and preservation of the fertility and productive power of normal soils are simple and well-established, and no state in the union can afford to ignore or belittle these great fundamental truths, nor to have the minds of its farmers and landowners befogged in relation thereto.

CYRIL G. HOPKINS
UNIVERSITY OF ILLINOIS

SCIENTIFIC BOOKS

The Living Plant. By WILLIAM F. GANONG. New York, Henry Holt and Co. 1913. 17 × 23 cm. Pages xii + 478; 3 colored plates; 178 figures, many in text. Price, $3.50.

This book aims to attract popular interest and at the same time to tell the truth about its subject. The work is avowedly not intended for scientists, but "it seeks to present to all who have interest to learn an accurate and vivid conception of the principal things in plant life" (preface). Thus the author has "been at more pains to be clear than to be brief," and the book "has wandered through a leisurely course to a length quite shockingly great" (preface). Nevertheless, the depth in natural science to which the reader is here carried is so great as to make it probable that the book will find its greatest use among those who already possess considerable knowledge of plants and their processes.

The style of the book combines clearness with personal frankness, the reader being taken into the author's confidence from the very first; it is a conversational style of the highest type, becoming even chatty at points, and generally maintaining a logical clearness and definiteness that is rare in popular or even elementary treatises upon such complex subjects. The language possesses a characteristic quaintness, almost an archaic tang at some points. A few examples of quite col-

loquial or even slang expressions may be noted. Regarding the numerous illustrations, they are exceedingly well chosen and well prepared, and they add markedly to the clearness of the exposition.

The author's attitude is conservative and many questions are left just as they should be, in a quite undecided condition. At the same time, the reader is admitted to some of the pleasures of hypothesis-making and of prophecy, generally in a very safe and clearly guarded manner, for the author has not hesitated to enliven his story and perhaps accelerate the advance of his science, by indulging in suggestions of scientific possibilities and probabilities.

"A table designed to display the plan of this book" is inserted after the table of contents, and exhibits a sort of synoptical outline of the subjects considered in the eighteen chapters, together with their logical connections. It is seldom that a book of this sort brings out as clearly as does the one before us the important relations of its various topics to each other and to human activities in general. Diagrams and tables are frequently resorted to. After a chapter on the ways in which plants appeal to human interest, seven chapters treat the material and energy transformations in the plant body. Then follows a chapter on irritability, one on "protection," two on reproduction, and two on growth. The four remaining chapters consider respectively, dissemination, evolution and adaptation, plant breeding and classification. This obviously very broad treatment comprises a sufficiency of new methods of presentation and novel placings of emphasis to make the book profitable reading for the research worker and the teacher as well as for the less advanced student.

Turning now to fault-finding, a few adverse criticisms may be noted as to use of words. The word *mani-colored* occurs in several places (*e. g.*, page 261); does not such a novelty suggest *handpainted?* Insectivorous plants are termed *insectivora* (page 104, for example); if the Latin form is employed we should prefer not to apply the neuter form to plants (*plantæ*). In these decadent days, as far as general interest in the ancient foundations of our language is concerned, it were perhaps better to cling to the perfectly safe but less euphonious English form, insectivores. The word *plenty* appears to be used throughout (*e. g.*, pages 140, 266) as a predicate adjective, where ordinary usage requires plentiful, plenteous or a word with some other root. To most scientists, and perhaps to most readers, these points may seem of little import, but the very excellence of the diction which characterizes this book as a whole renders its few shortcomings of this sort all the more outstanding.

As to the scientific matter itself, probably the only quite inadequate exposition occurs in connection with the discussion of capillarity (pages 180, 181), which, as it stands, seems to the reviewer logically quite hopeless. It is to be regretted that the author surrendered here to the suasion from his critic and forbore "to explain this interesting process in detail to the reader" (page 179).

All will agree with Professor Ganong, that any truthful chapter on protoplasm must "leave you with a very unsatisfied feeling" (page 164), but it does seem that the conception of this material might be clarified by the omission of the idea of "protoplasm *par excellence*" (page 143), letting the mixture of many substances stand for the present as the seat of the numerous, more or less peculiar processes which taken together make up life. If it pleases one's fancy to think that vitality is possessed by some single substance in protoplasm and that all other contained substances are to it merely environmental or conditional, no one can assert that such a view is illogical; this is purely a matter of feeling, over which we do not argue. But none can agree with the author, that "we are logically bound to believe that some such substance [as protoplasm *par excellence*] must exist as the seat of the distinctive properties of life" (page 143). Some people may be bound to believe this, but they are assuredly not *logically* so bound; nothing is now known of protoplasm which forces such an issue.

Likewise, it is difficult to find grounds for agreeing with the author when he states (page 199) that transpiration is a process "for which there is no equivalent in animals." Excepting when the higher animal is covered with water there is always more or less cuticular transpiration from its skin, just as there is in plants, and the wet membranes of the lungs and air passages are always transpiring large amounts of water into the internal atmosphere, just as happens in plant foliage and the like. Transpiration is a phenomenon common to all living things which are exposed to air, though its indirect effects are of course different in different forms.

A method of exposition to which many botanists will probably object, but which will no doubt receive the hearty approval of most physiologists, is the presentation of the entire subject of sexual reproduction without reference to the alternation of generations. From the dynamic point of view, it is surely desirable for an elementary treatise thus to omit the complicated story of sporophyte and gametophyte, megaspores and microspores. The reviewer looks upon this as a real stroke of genius, considering the dominance of these things in present-day botany.

Last, but not by any means least, among the points selected for mention here, is what may be termed the philosophy of the book before us. The whole presentation is frankly and insistently permeated with the peculiar confusion, so common in biological reasoning, of causes with effects; the account is written from the teleological standpoint. The author adds a new deity to the growing biological pantheon, thus developing "a perfectly natural vitalism based on the superior interpretative power of a hypothesis assuming the existence in nature of an *X*-entity, additional to matter and energy but of the same cosmic rank as they, and manifesting itself to our senses only through its power to keep a certain quantity of matter and energy in the continuous orderly ferment we call life" (page viii). To the purposefulness of this unknown Something are attributed the determining conditions that bring about the more complex phenomena of living things; wherever the physical antecedents or determining conditions of a phenomenon are not known (and they are mostly unknown in physiology), the hypothesis of the *X*-entity supplies a word with which to cloak our ignorance—as Professor Barnes used to say—and in this seems to lie the "superior interpretive power" of such hypotheses.

But this is not the place to add to the already great and bemuddled mass of academic argument concerning this present-day survival of the doctrine of special creation. Space may be taken to note further only three interesting aspects of the general philosophical attitude of "The Living Plant." First, non-teleologists will welcome the frankness and clearness with which the position of the author has been stated. While many teleologists explain the prevalent use of purposeful implications merely as verbal short-cuts, disavowing all belief in what the words actually state, and while such vague mental positions seem to give some weight to the accusation that it is but a "man of straw" against which the scientific monist directs his javelin, our present author makes it perfectly and unmistakably clear that he does hold to purpose as a logical cause of phenomena in matter and energy. Such clear statements must do much to clarify the atmosphere of this seemingly everlasting discussion.

The second interesting philosophical feature requiring some attention here is this, that along with the *deus ex machina* postulated to guide the threads through the active loom of time, and along with the common, every-day forces of the physical sciences, which seem to be conceived as keeping the loom in operation, there seems also to be (though the author does not definitely bring this out) a third force, or at least a third kind of factor, which takes part in conditioning phenomena, namely, *accident* or *chance*. One comes away from a careful reading of the entire presentation with a feeling that vital phenomena are brought about through the interaction of these three groups of directing conditions, the *X*-entity, nature and chance. No doubt the author will

agree, however, that chance is nothing but the very thing which emerges to some of us in his *X*-entity, just some complex of conditioning factors not yet known.

Finally, the book before us is pedagogically nearly ideal, and it may be that its teleological philosophy is one of its strong points in this regard. As the author will assuredly agree, scientific research is one thing and the teaching of science quite another; the elementary teacher does not try to tell the whole truth, but only those portions which may best lead on to such a state of mind in the student as will some time, perhaps, enable him to understand a large portion of the truth. Now, considering that physical causation is far too complex a subject even to be thought about adequately, until the thinking person has accumulated a vast store of accurate scientific experience, it may well follow that a perfectly monistic philosophy would not serve at all in an elementary treatise, and that a somewhat *devitalized* dualism is the only sort of inclined plane by which the scientifically untrained mind may be led toward the highest and clearest altitudes of scientific philosophy.

In conclusion, the book we have been considering is one of the American Nature Series, is bound in green cloth with a gilt-ornamental back, and is about 4 centimeters thick. It will always be read lying on the table. The paper stock is very heavy, clay-coated and highly surfaced, so that the numerous half-tone illustrations are exceedingly satisfactory. It is, however, also true that the position of the book and reader must be properly chosen to avoid dazzling high lights where the midnight lamp is reflected in the mirror-like surface of the paper. As with all such coated papers, a distinct odor of glue is perceptible throughout the reading; spattered water will play havoc with the pages.

B. E. Livingston

Studies in Luminescence. By Edw. L. Nichols and Ernest Merritt. Published by the Carnegie Institution of Washington, 1912. Royal 8vo, vi + 225 pp.

The memoir represents the results of investigation extending over a period of nine years. In large part it gives the experimental observations made by the authors; but in it are also observations on one or another phase of the general subject, made by other observers, mainly, however, under the guidance of the authors. The work has been aided by occasional grants of money from the Carnegie Institution of Washington, and the memoir is published by the institution. The material has been published previously in separate articles, most of which have appeared in the *Physical Review;* but it has now been given such continuity of form and (in the last two chapters of the memoir) such valuable theoretical discussion as to make the present publication one of unusual interest and value.

The authors, during these years, have evidently kept steadily before themselves the intention of using the spectrophotometer to the farthest possible extent. The success with which they have held to such intention, in investigations of a dozen or so of luminescent substances, is nothing short of remarkable. Measurements of intensities have been carried out far toward the edges of fluorescent and phosphorescent bands. In the cases of nearly all substances investigated, measurements were made to determine the exact form and extent of absorption bands corresponding to given luminescence bands. The dependence of the intensities of luminescence upon the wave-length of exciting light, and the distribution of intensities for some substances when excited by Röntgen rays and by cathode rays, have been studied. More remarkable still is the extent to which the spectrophotometer has been used in following the decay of phosphorescence at various wave-lengths in chosen bands. When one considers how weak the illuminations in the comparison fields of this instrument are, at the limits of a band or after some time of decay, the range of application which the method finds is surprising. Numerous settings were made with intensities in the comparison fields so small as to convey to the observer no sensation of color. The concord-

ance of results, obtained under widely varying conditions, bespeaks the care and patience with which unavoidable errors in individual readings have been ironed out by the law of averages.

In following the decay of phosphorescence there is obviously a stage beyond which the spectrophotometer, on account of its wastefulness and dispersion of light, is no longer applicable. In working beyond such limits with apparatus adapted to these researches from the methods of ordinary or gross photometry, Professors Nichols and Merritt took every care to excite only that band with which they were at the time concerned. Where in a few cases this precaution could not be observed, great care was exercised to insure the desirable uniqueness of significance for the results. Two very important laws, established from the spectrophotometric measurements, add much to the significance of such measurements as were of necessity made by gross photometry. The laws express the individuality of behavior of a given luminescence band as a whole, throughout a wide range of conditions of excitation, and, in the case of phosphorescence, during decay. A band maintains measurably the same relative distribution of intensities and the same wave-length of maximum intensity. As the authors express it "the band behaves as a unit." It is, now, not unreasonable to assume that, beyond the limits of availability of spectrophotometric methods, a given luminescence band still behaves as a unit, and that the measurements made thereafter on the band as a whole should indicate with fair certainty how the intensities at the individual wave-lengths decay.

Stokes's law of photo-fluorescence, namely that the fluorescent light is of greater wave-length than the exciting light, is verified in its gross sense. But since in a large number of instances, the corresponding luminescence and absorption bands overlap, and since the whole of a given band may be excited by light of any wave-length within the region of absorption, it follows that, considered in detail, the law frequently fails.

To these three important laws, and another concerning the absence of polarization in the luminescence spectrum even when it is excited by polarized light, the authors add, from their own results and from those of other observers, some general "facts connected with the decay of phosphorescence." These are—the form of the curve of decay; the hysteresis effect or the dependence of the form of decay curve upon the previous excitation to which the substance has been subjected; the effect of red and infra-red rays (ingeniously used to restore a substance to a standard condition after each excitation); and the effect of high temperatures.

Much work was done in the study of electrical properties of fluorescent solutions. This and the efforts of other investigators in the same field has led generally to negative results in the search for change in electrical resistance of solutions during fluorescence.

Important also is the work done to reduce the initial observations, made by spectrophotometer with diffused light of the acetylene flame as a comparison standard, to the fundamental basis of energy curves, and that which was done to determine the specific exciting power (intensity of fluorescence excited per unit of absorbed energy) of various wave-lengths of exciting light.

The last two chapters of the memoir are devoted to the consideration of theories by means of which the experimental data thus far gathered may be related and explained. The discussion is notably interesting and lucid throughout. Chapter XIV. gives an outline of the dissociation theory of Wiedemann and Schmidt, and shows that it has already been applied with considerable success to the explanation of fluorescence in gases. In Chapter XV. the authors add such other hypotheses as would seem necessary to make this theory specifically applicable to the problem in hand, and deduce therefrom laws which follow experimental results with surprising success—remarkable, indeed, when one considers how complex must be the processes which occur in luminescent solutions, solid and liquid.

One of the valuable things accomplished by a memoir such as the one in review, collecting

in orderly form and discussing as it does a vast amount of material of observation, is the pointing out of gaps in available data. The pages of these "studies" raise numerous questions which must be settled in order that the whole fabric of luminescence theory may be further extended. Undoubtedly many of the questions so raised will be worked out in the same laboratories from which the present researches have been issued.

Even thus far the work represented in the present memoir constitutes a most noteworthy series of researches in the general field of luminescence. Not only the care and patience with which the observations have been made, but much more the experimental acumen with which the methods and materials have been chosen and the illuminating discussions of theoretical character, all contribute to give these researches a place beside those of the middle of the past century by which E. Becquerel blazed the way into this wonderfully interesting region. Recent developments in physics attach much more of importance to the phenomena of luminescence than could possibly have been foreseen in those earlier years, and it seems certain that further developments, in this and allied branches of physics, will greatly enhance the value of the region as a field for research.

F. E. Kester

SPECIAL ARTICLES

NON-ELECTROLYTES AND THE COLLOID-CHEMICAL THEORY OF WATER ABSORPTION

The colloid-chemical theory best explains at the present time the absorption of water by protoplasm under various physiological and pathological conditions. The laws governing the absorption of water by such simple protein colloids as fibrin and gelatine, are point for point identical with those which we have known to govern the absorption of water by cells, tissues, organs, or the organism as a whole. Thus fibrin and gelatine swell more in any acid solution than in distilled water, while protoplasm does the same. The addition of any salt to the acid solution reduces the amount of this swelling, and this the more the higher the concentration of the salt. The same holds for protoplasm. At the same concentration different salts arrange themselves in a characteristic order in this regard. The same order is observed in protoplasm.

In this way it has been possible to explain without contradiction not only all those phenomena which are ordinarily said to prove the tenability of the laws of osmotic pressure for the processes of absorption and secretion, as observed in protoplasm under various pathological and physiological circumstances, but also the notable exceptions in behavior, which no one believes explainable on the osmotic basis.

In the study of fibrin and gelatine, it was found that various non-electrolytes, such as sugars and alcohols, are *relatively* ineffective in reducing their swelling in the presence of any acid. Adherents to the osmotic theory of water absorption have made this statement read, *entirely* ineffective; and, because certain non-electrolytes bring about shrinking effects in various cells and tissues, have seen in this a valid reason for rejecting the dominant importance of the colloids and their state in determining the amount of water held by protoplasm.

During the past year a systematic study of the effect of various non-electrolytes on the swelling of gelatine and fibrin has been undertaken. *The effect of non-electrolytes upon these is identical with their effect upon protoplasm.* The various organic compounds thus far studied (saccharose, dextrose, levulose glycerine, ethyl alcohol, methyl alcohol, propyl alcohol, propylene glycol, etc.) all decrease the swelling of gelatine or fibrin in either neutral or acid solution, and this the more the higher the concentration of the added compound. When equally concentrated (equimolecular) solutions are compared the sugars are found to be more effective than the alcohols in this regard. The same is true of protoplasm. The sugars among themselves are unequally effective in dehydrating protein colloids, and in a similar way are they unequally effective in dehydrating living tissues.

We have defined the excess of water held by tissues under various abnormal circumstances and known under the varying names of excessive turgor, plasmoptysis, edema, etc., as a state of excessive hydration of the tissue colloids, more particularly of the proteins. As the causes of this we have assigned any substance or condition which, under the circumstances surrounding the living cell, is capable of increasing the hydration capacity of its colloids. As the most potent of these causes we have regarded, and still regard, an abnormal production or accumulation of acid in the involved cell. Of other substances conceivably active in certain tissues, which thus increase the hydration capacity of the tissue colloids, we have studied urea. The addition of urea increases in all concentrations the swelling of gelatine and fibrin, and this the more the higher the concentration of the urea. In the higher concentrations of urea both gelatine and fibrin are hydrated so heavily that they go into solution. The urea hydration is not a simple alkali effect, for acid in no concentration counteracts it. The hydrating effects of acid and of urea are additive. There is, however, an interesting difference between the increased hydration brought about by acids and that induced by urea. While salts reduce the former, they do not affect the increased hydration induced through urea. On the other hand, various non-electrolytes which affect the hydration brought about by acids but little, reduce that produced by urea almost entirely.

These facts, taken in conjunction with our previous studies on the colloid-chemistry of absorption and secretion, help toward an interpretation of certain well-known biological and medical facts. They explain on a colloid-chemical basis the behavior of the sugars and certain other organic substances in reducing the absorption of water by tissues. They explain the cathartic action of glycerine, the sugars, etc. They also explain the diuretic action of these substances, accounting for the polyuria of diabetes, the relative dryness of the diabetic's tissues and his thirst. We get an insight into the mechanism of urea hemolysis. Also we learn another method of dehydrating edematous tissues, which owe their excessive hydration to other circumstances than the presence of acid or the absence of salts. In addition to using sugar in order to correct the "acidosis" of certain pathological states from a biochemical point of view, we have made practical use of the above facts by using sugar along with the alkali and hypertonic salt mixtures previously recommended in combating the edemas of the eye (glaucoma), brain (uremia), medulla (pernicious vomiting), kidney (nephritis), and other organs observed in various clinical conditions. The use of dextrose along with salts and alkali in these conditions has yielded even better results than have previously been reported.

A series of papers submitted for publication in the *Kolloid Zeitschrift* will shortly bring the details of these various findings.

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CHANGES DURING QUIESCENT STAGES IN THE METAMORPHOSIS OF TERMITES

THERE have been several theories as to when the larvæ of termites become differentiated to the various castes in the social organization, the prevalent theory being that undifferentiated larvæ are developed to the castes by the character of the food that they receive. The results of Heath's[1] experiments, however, to determine the relation of various kinds of food to polymorphism were negative. In case of the ants, Wheeler[2] with Emery believes "the adult characters to be represented in the germ as dynamical potencies or tensions rather than morphological or chemical determinants" and that "nourishment, temperature and other environmental factors merely furnish the condi-

[1] Heath, H., "The Habits of California Termites," *Biol. Bull.*, Woods Hole, Vol. IV., December, 1902, pp. 47–63.

[2] Wheeler, W. M., "The Polymorphism of Ants," *Bull. Amer. Mus. Nat. Hist.*, Vol. XXIII., January, 1907, pp. 1–93.

tions for the attainment of characters predetermined by heredity." Bugnion,[3] studying *Eutermes lacustris* and *Termes Redemanni* Wasm. and *Horni* Wasm. states that the differentiation takes place during the embryo stage for the three castes, rather than undifferentiated larvæ being developed to the castes by the character of the food they receive.

Observations by the writer of molting soldier larvæ of *Leucotermes* spp. and *Termopsis angusticollis* Walk. show that the differentiation takes place during a "quiescent"[4] stage rather late in the life cycle. At this point a brief outline of the life cycle is necessary.

In the metamorphosis of the above species the eggs hatch into active, undifferentiated larvæ which develop to the various mature forms or castes by a gradual growth through a series of molts and quiescent stages. During the quiescent stage both the larvæ and nymphs pass through an inactive period, of comparatively short duration, isolated, lying on the side, head bent down to lie on the ventral side of the body along which the antennæ and legs also lie extended in a backward direction. The writer first observed molting larvæ in a quiescent stage on August 11, 1911, in a colony near Jerseyville, Illinois. During April, 1912, the development of nymphs of the first and second forms of *Leucotermes flavipes* Kol. and *virginicus* Banks was observed at Falls Church, Virginia, and it was noted that both these nymphs passed through a quiescent stage in the final molt to the reproductive forms; nymphs of *Termopsis angusticollis* Walk. also pass through this quiescent stage. From the first to the middle of August, 1913, freshly molted, pigmentless soldiers of *flavipes* in the stage preceding maturity were noticeable in colonies in Virginia. On August 17, 1913, molting soldier larvæ were found in the quiescent stage in a colony of *virginicus* at Chain Bridge, Virginia. During the quiescent stage differentiation took place. Larvæ to all external appearances undifferentiated or of the worker type (as shown by the head, the mandibles—with marginal teeth—and the labrum of the still adhering larval skin), the individuals (*virginicus*) being over 3 mm. in length in the quiescent condition, antennæ with 14 segments, develop at this molt to pigmentless nymphs of soldiers with more elongate, soldier-like head and saber-like mandibles, without marginal teeth. In this stage the head, mandibles, labrum and "menton" (Bugnion) have not attained the shape or length of those of the mature soldier, there being at least one later molt to maturity.

Therefore, it may be stated that in case of *Leucotermes* spp. and *Termopsis angusticollis* Walk., the differentiation of the soldier caste occurs during a molt and quiescent stage rather late in the life cycle of the insect, the larvæ being previously, to all external appearances, undifferentiated.

Thomas E. Snyder
Bureau of Entomology,
Branch of Forest Insects,
September 11, 1913

[3] Bugnion, Pr. E., "La différenciation des castes chez les Termites," *Bull. de la Société entomologique de France*, No. 8, April, 1913, pp. 213–18.

[4] Strickland, E. H., "A Quiescent Stage in the Development of *Termes flavipes* Kol.," *Journ. N. Y. Ent. Soc.*, Vol. XIX., No. 4, December, 1911, pp. 256–59.

THE AMERICAN MATHEMATICAL SOCIETY

The twentieth summer meeting and seventh colloquium of the American Mathematical Society were held at the University of Wisconsin during the week September 8–13, 1913. The attendance, which exceeded that of any previous summer meeting of the society, included fifty-seven members. The four sessions of the summer meeting proper, for the presentation of papers, occupied the first two days of the week. The first session opened with an address of welcome by Professor C. S. Slichter in behalf of the University of Wisconsin and the local members of the society. The president of the society, Professor E. B. Van Vleck, occupied the chair at this and at the final session. Professor Oskar Bolza presided at the second, and Professor W. F. Osgood at the third session. The council announced the election of the following persons to membership in the society: Mr. W. E. Anderson,

Princeton University; Professor W. O. Beal, Illinois College; Dr. C. A. Fischer, Columbia University; Professor A. E. Landry, Catholic University of America; Lieutenant Salih Mourad, Ottoman navy; Miss E. A. Weeks, Mount Holyoke College. Thirteen new applications for membership were received.

It was decided to hold the summer meeting of 1915 at San Francisco in connection with the Panama Exposition. The secretary reported that a separate office for the society had been provided by Columbia University and that the services of a clerk had been engaged for carrying on the considerable routine work of the secretary, treasurer, librarian, committee of publication and shipping office. It was decided to issue the Register of the society hereafter at intervals of two or three years; in the intervening years only a mere list of officers and members will be published. Professor L. E. Dickson was appointed editor-in-chief of the *Transactions*, the other members of the editorial committee being at present Professors H. S. White and D. R. Curtiss. The society has recently published the Princeton Colloquium Lectures delivered at the sixth colloquium in 1909 by Professor G. A. Bliss on "Fundamental existence theorems" and Professor Edward Kasner on "Differential-geometric aspects of dynamics."

The arrangements made by the local committee for the comfort and entertainment of the members throughout the week were perfect. No place in the middle west could be more ideal for such a series of meetings than Madison. The spacious lecture halls of the university, the beautiful campus occupying an elevated position overlooking the capitol building and the adjacent lakes, Mendota and Monona, the commodious University Club used as headquarters, and the hospitality of President Van Vleck and other members of the faculty who opened their homes for the entertainment of the members—these and many other items contributed to the success of the farthest west summer meeting and only western colloquium.

On Monday evening President Van Vleck entertained at dinner the members of the council and the colloquium lecturers. On Wednesday afternoon the committee provided a two-hours' special excursion on Lake Mendota, ending at the Golf Club House in time for the dinner, at which fifty-five persons sat down. President Van Vleck acted as toastmaster and informal speeches were made by Professors Osgood, Bolza, Moore, Blichfeldt, Dickson and Dr. Jackson. A telegram was sent to the secretary, expressing appreciation of his services to the society and great regret at his enforced absence. At the close of the dinner Professor Ziwet voiced the unanimous sentiment in expressing thanks to the university and the committee on arrangements for their generous hospitality. The dinner was followed by a moonlight ride on the lake back to the University Club. On Thursday the members were conducted by Professor Skinner about the campus and buildings of the university; and on Friday an automobile ride was provided by the mathematical faculty and their friends, giving the members a fine opportunity to see the immediate surroundings of Madison. This ended in a most enjoyable buffet dinner at the home of President Van Vleck.

The following papers were read at the four sessions of the summer meeting:

E. B. Lytle: "Note on iterable fields of integration."

W. H. Bussey: "The tactical problem of Steiner."

Josephine E. Burns: "The abstract definitions of the groups of degree eight."

William Marshall: "The functions of the parabolic cylinder."

L. C. Karpinski: "The algorism of John Killingworth."

R. D. Carmichael: "On series of iterated linear fractional functions."

R. D. Carmichael: "Some theorems on the convergence of series."

T. E. Mason: "The character of the solutions of certain functional equations."

E. B. Van Vleck and F. T. H'Doubler: "On certain functional equations."

Oskar Bolza: "On the so-called 'abnormal' case of Lagrange's problem in the calculus of variations."

E. R. Hedrick and W. D. A. Westfall: "An existence theorem for implicit functions."

R. G. D. Richardson: "A solution of the Rayleigh minimum problem in the theory of sound."

G. C. Evans: "The Cauchy problem for integro-differential equations."

Dunham Jackson: "A formula for trigonometric interpolation."

J. W. Alexander, II.: "Proof of the invariance of certain constants in analysis situs."

J. E. Rowe: "On Fermat's theorem and related theorems (first paper)."

J. E. Rowe: "On Fermat's theorem and related theorems (second paper)."

Maxime Bôcher: "The infinite regions of various geometries."

W. F. Osgood: "On functions of several variables which are meromorphic or analytic at infinity."

W. F. Osgood: "Note on line integrals on an algebraic surface $f(x, y, z) = 0$."

E. H. Moore: "On a class of continuous functional operations associated with the class of continuous functions on a finite linear interval (preliminary communication)."

A. R. Schweitzer: "On a general category of definitions of betweenness."

A. R. Schweitzer: "The theory of linear vectors in Grassmann's extensive algebra."

A. R. Schweitzer: "Remarks on functional equations."

A. R. Schweitzer: "The general logical significance of uniformity of convergence of series."

Edward Kasner: "On the ratio of the arc to the chord for analytic curves."

E. L. Dodd: "The arithmetic mean as approximately the most probable value *a posteriori* under the Gaussian law."

E. J. Wilczynski: "On the surfaces whose directrix curves are indeterminate."

J. B. Shaw: "On the transverse of a linear vector operator of n dimensions."

Florian Cajori: "Zeno's arguments on motion."

O. E. Glenn: "Note on a translation principle connecting the invariant theory of line congruences with that of plane n-lines."

F. R. Sharpe: "Conics through inflections of self-projective quartics."

F. R. Sharpe and C. F. Craig: "Plane curves with consecutive double points."

Mildred L. Sanderson: "A method of constructing binary modular covariants."

H. M. Sheffer: "Superpostulates: introduction to the science of deductive systems."

H. M. Sheffer: "A set of six independent postulates for Boolean algebras."

R. M. Winger: "Self-projective rational sextics."

R. M. Winger: "Self-projective rational septimics" (preliminary report).

M. Fréchet: "Sur la notion de differentielle d'une fonction de ligne."

Kurt Laves: "A new theorem concerning the motion of two satellites of finite masses circulating in nearly commensurable motions of type $\frac{1}{2}$ about a central and homogeneous body of ellipsoidal shape."

H. F. Blichfeldt: "On the order of linear homogeneous groups (fifth paper)."

T. R. Running: "Graphical solutions of differential equations between two variables."

R. P. Baker: "The genus of a group."

R. P. Baker: "The topological configurations occurring in finite geometries."

R. D. Carmichael: "On Fermat's theorem and related theorems."

H. W. March: "Integral and series representations of an arbitrary function in terms of spherical harmonics."

The colloquium opened on Wednesday morning and occupied the rest of the week. Two courses of five lectures each were given by Professor L. E. Dickson on "Certain aspects of a general theory of invariants, with special consideration of modular invariants and modular geometry," and Professor W. F. Osgood on "Topics in the theory of functions of several complex variables." Printed syllabi of the lectures had been distributed in advance of the meeting. Fifty-one persons attended the lectures, a larger number than at any previous colloquium. An abstract of the lectures will be published in the *Bulletin* of the society.

The next meeting of the society will be held at Columbia University on Saturday, October 25. The San Francisco Section will meet on the same day at Stanford University. The annual meeting of the Southwestern Section will be held at the University of Missouri on Saturday, November 29.

H. E. Slaught,
Acting Secretary